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ESSAYS, MONOGRAPHS, AND CASES.

Anatomy of the Placenta. By JOHN C. DALTON, M.D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons of New York.

The object of the present paper is to demonstrate, by positive evidence, some points in the anatomy of the placenta which are still in dispute. It is more particularly with regard to the precise mode of vascular connection existing between the placenta and the uterus, that there are some discrepancies among the anatomists of the present day; and I shall now ask the attention of the Academy to the result of certain experiments which I have recently performed, and which seem to leave no doubt with regard to the real nature of this connection.

Ever since the time of the Hunters, it has been believed by many that the blood-vessels of the uterus were directly prolonged into the substance of the placenta. William Hunter, with the assistance of his brother John, injected, in several instances, the vessels of the pregnant uterus with wax, and found, that although none of the injection passed into the vessels of the umbilical cord, it nevertheless found its way freely into the interior of the placenta. To use his own words, "the cells or interstices in the spongy portion of the pla-

centa were universally loaded with wax; either the blue, which was injected into the veins of the womb, or the red, which was thrown into the arteries."

He also says: "Most of the wax, which was first injected by the veins of the womb, was driven on toward the internal surface (of the placenta); and the red wax, which was afterward injected by the arteries, was lodged, principally, in the outer parts; but the two colors were more or less blended through the whole."

These injections are represented in Hunter's plates of the gravid human uterus, number five and number twenty-four.

In 1833, Mr. Mayo and Mr. Stanley, of London, examined some of Hunter's injected preparations of the placenta, preserved in the Museum of the Royal College of Surgeons, and made a report of their examination in the *London Lancet* for June 22d of the same year. They found that the umbilical arteries had been injected in yellow, the uterine arteries in red, and the uterine veins in black; and that the injection of the uterine vessels had evidently passed into the whole thickness of the placenta.

About the year 1832, Prof. Weber, of Leipzig, also made a double injection of the uterus and placenta, filling the substance of the placenta with colored wax from the uterine vessels. This specimen is still preserved in Prof. Weber's Museum, connected with the Medical School of Leipzig.

The above are the only instances, so far as I am aware, in which such injections have been made, since the commencement of the present century. They led to the belief, among a certain number of the profession, that the maternal and fetal vessels were intermingled in the placenta—the injection in both instances being supposed to have followed the natural course of the blood. They were not, however, universally regarded as conclusive, and very different views have been since maintained on this point by men of the first eminence in the profession.

Dr. Robert Lee, in the *London Lancet* for April 20th, 1833, published an account of some dissections which he made of pregnant uteruses under water, from which he derives the conclusion that there is no such penetration of the uterine vessels into the placenta, imagined by the Hunters and by Prof. Weber. He regards the filling of the placenta in these instances by the wax injection as entirely an effect of extravasation; and maintains that the sinuses of the uterus do not penetrate beyond the uterine or decidual surface of the placenta.

"When air," he says, "is forcibly thrown either into the spermatie

arteries or veins, the whole inner membrane of the uterus is raised by it; but none of the air passes across the deciduous membrane into the placenta, nor does it escape from the semilunar openings in the inner membrane of the uterus, until the attachment of the deciduous membrane to the uterus is destroyed. There are no openings in the deciduous membrane corresponding with those valvular apertures now described in the internal membrane of the uterus."

I would ask attention more particularly to the above statements of Dr. Lee, as to the non-injection of air from the uterus to the placenta, since they are in direct opposition with the results of my own experiments, to be described presently.

Dr. Lee goes on to say, "the facts which have now been stated warrant, I think, the conclusion that the human placenta does not consist of two parts, maternal and fetal; that no cells exist in its substance, and that there is no communication between the uterus and the placenta by large arteries and veins."

Dr. John Reid, on the other hand, in 1841 published* an elaborate account of the dissection of a pregnant uterus, with the placenta attached, in which he comes to an opposite conclusion, and maintains that the uterine vessels not only penetrate into the placenta, but extend throughout its entire thickness, enveloping everywhere the fetal tufts. He was first led to adopt this view, by noticing that some of the placental tufts extended entirely through the decidual surfaces of the placenta, and projected more or less into the sinuses in the muscular walls of the uterus. No injection was practised by Dr. Reid, excepting that of the umbilical arteries, for the purpose of ascertaining the arrangement of vessels in the fetal tufts, and his conclusions with regard to the disposition of the maternal sinuses on the placenta were derived altogether from careful dissection. He accordingly speaks of his results with a considerable degree of confidence, though not, as we shall see, with absolute certainty.

"The outer surface of the placental vessels," he says, "has a smooth appearance, and they are, *we may suppose*, everywhere enveloped in the inner coat of the vascular system of the mother, which, as we have seen above, is reflected upon them." "The interior of the placenta is thus composed of numerous trunks and branches, (each including an artery and an accompanying vein,) every one of which, we believe, is closely ensheathed in prolongations of the inner coat of the vascular system of the mother, *or at least in a membrane continuous with it*. If we adopt this view of the structure of the placenta," he

* Edinburgh Medical and Surgical Journal, January, 1841.

goes on to say, "the inner coat of the vascular system of the mother is prolonged over each individual tuft, so that when the blood of the mother flows into the placenta through the curling arteries of the uterus, it passes into a large sac formed by the inner coat of the vascular system of the mother, which is intersected in many thousands of different directions, by the placental tufts projecting into it like fringes, and pushing its thin wall before them in the form of sheaths, which closely envelope both the trunk and each individual branch comprising these tufts. From this sac the maternal blood is returned by the utero-placental veins without having been extravasated or without having left her own system of vessels."

In 1845, Prof. John Goodsir, of Edinburgh, corroborated Dr. Reid's views, by the results of microscopic examination of the substance of the placenta and decidua membrane;* and since that time these views have been received with more general favor by the profession than those of an opposite character.

Very recently, however, they have been again denied *in toto* by an observer of at least equal eminence with those already mentioned. M. Ch. Robin, of Paris, has published in the first number of the *Journal of Physiology*, conducted by M. Brown-Séquard, a paper entitled *Anatomy and Physiology of the Uterine Mucous Membrane, and its Epithelium during Gestation*, in which he maintains that the maternal sinuses cease at the uterine surface of the placenta, and do not penetrate into its substance. "There are no other utero-placental vessels existing," he says, "than those which are contained in the folds or processes of the uterine mucous membrane dipping down, in the mammalian animals above mentioned, into the spaces between the villousities of the chorion; and in the human subject, into the shallow furrows between the lobules of the placenta."

M. Robin also attributes, like Dr. Lee, all the instances of injections of the placenta from the uterine vessels to a rupture of the latter and consequent extravasation, the ruptures being, as he says, easily detected where they exist.

M. Robin occupies, as I have already intimated, the very first rank among physiologists and minute anatomists at the present day. I do not say this for the purpose of adding anything to his reputation, but merely in order to show that I appreciate fully the weight of his opinion, and that I should not maintain views directly opposed to his, as I shall do in the present paper, without having been personally convinced upon testimony of the most complete and satisfactory nature.

* British and Foreign Medical Review, October, 1845.

I feel confident, indeed, from the facts which I shall immediately mention, that the blood-vessels of the uterus *do* really penetrate into the substance of the placenta, as supposed by the Hunters, Dr. Reid, and Prof. Goodsir, and that they constitute, with the tufts of the foetal chorion, an equal part of its mass.

The placenta is, therefore, a double organ, partly maternal and partly foetal; and, in order to arrive at a distinct understanding of the arrangement of its vessels, I will first go through with a hasty description of the development of the two structures which enter principally into its formation, viz: First, the chorion of the foetus, and secondly, the decidual membrane of the uterus.

I.—*Of the Chorion.* The external investing membrane of the egg as it comes from the ovary, is at first perfectly smooth. But after the formation of the chorion, and particularly after the blood-vessels of the allantois begin to extend into it, it becomes shaggy. Its exterior is seen to be covered with little transparent prominences, like so many villi, which increase the extent of its surface, and assist in the absorption of fluids from without. The villi are, at this time, quite simple in form, and homogeneous in structure.

As the egg increases in size, these villi rapidly elongate, and become, at the same time, divided and ramified by the repeated budding and sprouting of lateral off-shoots from every part. After this process of growth has gone on for some time, the external surface of the chorion presents a uniformly velvety or shaggy appearance, owing to its being covered everywhere with these tufted and compound villousities.

The villousities themselves, when examined by the microscope, have an exceedingly well-marked and characteristic appearance. They originate from the surface of the chorion by a somewhat narrow stem, and divide into a multitude of secondary and tertiary branches of varying size and figure; some of them slender and filamentous, others club-shaped, many of them irregularly swollen at various points. All of them terminate by rounded extremities, giving to the whole tuft a certain resemblance under the microscope to some kinds of sea-weed. The larger trunks and branches of the villosity are seen to contain numerous rounded or oval nuclei, imbedded in a nearly homogeneous or finely granular substratum. The smaller villousities appear, under a low magnifying power, simply granular in texture.

While the villousities just described are in process of formation, the allantois itself has completed its growth, and has become converted into a permanent chorion. The blood-vessels coming from the umbil-

ical arteries accordingly ramify over the chorion, and supply it with a tolerably abundant vascular net-work. These vessels penetrate also into the substance of the villousities. They enter the base or stem of each tuft, and, following every division of its compound ramification, they reach, at last, its rounded extremities. Here they turn upon themselves in loops, and retrace their course, to unite finally with the venous branches which empty into the umbilical vein.

Afterward, the chorion, which is, at the period above described, uniformly villous, shaggy, and vascular, becomes, in the progress of its development, partially bald. This change, which begins to take place about the end of the second month, commences at a point opposite the situation of the fœtus—and the insertion of the umbilical cord. The villousities in this region cease growing, and as the entire egg continues to enlarge, the villousities at the point indicated fail to keep pace with its growth and with the progressive expansion of the chorion. They accordingly become, at this point, thinner and more scattered, leaving that part of the surface of the chorion comparatively smooth and bald. This baldness increases in extent, and becomes more and more complete, spreading and advancing over the adjacent portions of the chorion, until at least two-thirds of its surface have become nearly or quite destitute of villousities.

At the opposite portion of the egg, at the same time, that portion, namely, which corresponds with the insertion of the umbilical cord, the villousities, instead of becoming atrophied, continue to grow, and this part of the chorion becomes constantly more shaggy and thickly set than before. The consequence is, that the chorion afterward presents a very different appearance at different portions of its surface. The greater part is smooth, but a certain portion, constituting about one-third of the whole, is covered with a soft and spongy mass of long, thickly set, compound villousities. It is this thickened and shaggy portion which is afterward concerned in the formation of the placenta; while the remaining smooth portion continues to be known under the name of the chorion.

The vascularity of the chorion keeps pace in its different parts respectively with the atrophy and development of its villousities. As the villousities shrivel and disappear over a part of its extent, the looped capillary vessels, which they at first contained, disappear also, so that the smooth portion of the chorion shows afterwards only a few straggling vessels running over its surface, and does not contain any abundant capillary plexus. In the thickened portion, on the other hand, the vessels lengthen and ramify to an extent corresponding with the

villosities in which they are situated. The umbilical arteries, coming from the abdomen of the fœtus, enter the villi and penetrate through their whole extent, forming at the placental portion of the chorion a mass of tufted and ramified vascular loops; while over the rest of the membrane they are merely distributed as a few single and scattered vessels.

II.—*Of the Decidua.* The decidua, which constitutes at first the remaining portion of the placenta, is nothing more than the mucous membrane of the body of the uterus. This membrane in the unimpregnated condition is soft and delicate in texture, and presents a smooth and slightly vascular internal surface. It consists throughout of minute glandular tubules, ranged side by side, and running perpendicularly to the free surface of the mucous membrane, occupying its entire thickness, and resting by their rounded extremities upon the subjacent muscular tissues; while their mouths open into the cavity of the uterus. A few fine blood-vessels penetrate the mucous membrane from below, and, running upward between the tubules, encircle their superficial extremities with a capillary net-work. There is no areolar tissue in the uterine mucous membrane, but only a small quantity of spindle-shaped fibro-plastic fibres, scattered between the tubules.

As the fecundated egg is about to descend into the cavity of the uterus, the mucous membrane, just described, takes on an increased activity of growth. It becomes tumefied and congested; and as it increases in thickness, it projects in rounded eminences or convolutions into the uterine cavity. The tubules increase in size, and the blood-vessels of the mucous membrane at the same time enlarge and multiply, and inosculate freely with each other; so that the vascular net-work encircling the tubules becomes more extensive and abundant.

The internal surface of the uterus, therefore, after this process has been for some time going on, presents a thick, rich, soft, vascular and velvety lining, which is the decidua, and which is thrown into abundant projecting folds and convolutions.

As the fecundated egg, in its journey from above downward, passes the lower orifice of the fallopian tube, it insinuates itself between the opposite surfaces of the uterine mucous membrane, and becomes soon afterwards lodged in one of the furrows or depressions between the projecting convolutions of the decidua. It is at this situation that an adhesion will subsequently take place between the external membrane of the egg on the one hand, and the uterine decidua on the other. At the point where the egg becomes fixed and entangled in this manner, a still more rapid development than before takes place in the uterine

mucous membrane. Its projecting folds begin to grow up around the egg, and enveloping it more and more, at last close together over its most prominent portion, enclosing it in this way in the decidua reflexa.

Let us now see in what manner the egg becomes attached to the decidua membrane. While the changes just noticed are taking place in the walls of the uterus, the growth of the egg and the development of the chorion have been going on simultaneously. The projecting filaments or villousities of the chorion, which are at first solid and non-vascular, insinuate themselves as they grow, either into the uterine tubules or between the folds of the decidua surface with which the egg is in contact, penetrating in this way into little cavities or follicles of the uterine mucous membrane. When the formation of the decidua reflexa is completed, the chorion has already become uniformly shaggy, and its villousities, spreading in all directions from its external surface, penetrate everywhere into the follicles just described. In this way the egg becomes entangled with the decidua—both decidua vera and decidua reflexa, throughout the whole extent of its surface.

Soon afterward the umbilical vessels, which emerge from the body of the fœtus to ramify in the chorion, penetrate everywhere into the villousities of that membrane, and become fully developed. Each villosity of the chorion then, as it lies imbedded in its uterine follicle, contains a vascular loop, through which the fetal blood circulates, increasing in this way the activity of absorption and exhalation.

Subsequently, as we have seen, these vascular tufts, which are at first uniformly abundant throughout the whole extent of the chorion, disappear over a portion of its surface; while at the same time they become concentrated, and still further developed at a particular spot, the situation of the future placenta. This is the spot at which the egg is in contact with the decidua vera. Here, therefore, both the decidua membrane and the tufts of the chorion continue to increase in thickness and vascularity; while elsewhere, over the prominent portion of the egg, the chorion not only becomes bare of villousities and comparatively destitute of vessels, but the decidua reflexa, which is in contact with it, also loses its activity of growth, and becomes expanded into a thin layer, nearly destitute of vessels, and without any remaining trace of tubules or follicles.

The placenta accordingly is formed by the continued growth at one particular spot of the villi of the chorion, and the follicles of the decidua. The uterine follicle, into which the villus has penetrated, enlarges to a similar extent with the latter, sending out branching diverticula, corresponding with the multiplied ramifications of the villus. In fact,

the growth of the follicle and that of the villus go on simultaneously, and keep pace with each other; the latter constantly advancing as the cavity of the former enlarges.

But it is not only the *follicles* of the uterine mucous membrane which increase in size at this period. The capillary blood-vessels, which lie between them and ramify over their exterior, also become unusually developed. They enlarge and inosculate more freely with each other, so that every uterine follicle is soon covered with an abundant net-work of dilated capillaries, derived from the blood-vessels of the original decidua. At this time, therefore, each vascular loop of the foetal chorion is covered first with a layer forming the wall of the villus. This is in contact with the lining membrane of the uterine follicle, and outside of this again are the capillary vessels of the uterine mucous membrane; so that two distinct membranes intervene between the walls of the foetal capillaries on the one hand, and those of the maternal capillaries on the other; and all transudation must take place not only through the walls of the vessels, but also through the substance of these two membranes intervening between them.

As the formation of the placenta goes on, the general anatomical arrangement of the foetal vessels remains the same. These vessels continue to form vascular loops, penetrating deeply into the substance of the decidua; only they become constantly more elongated, and their ramifications more abundant and tortuous.

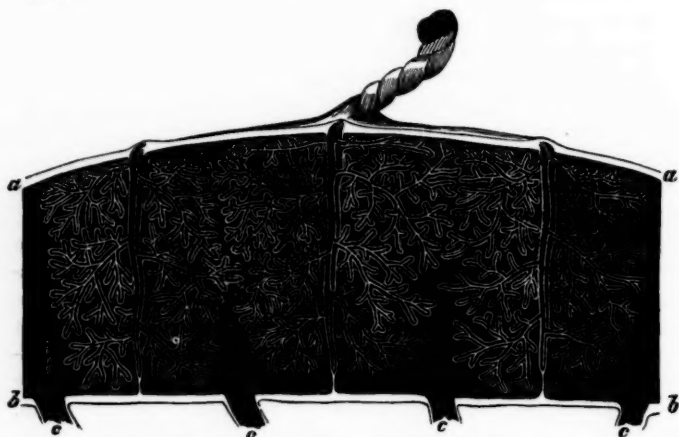
The maternal capillaries, however, situated on the outside of the uterine follicles, become considerably altered in their anatomical relations. They enlarge excessively, and, by encroaching constantly upon the little islets or spaces between them, fuse successively with each other; and, losing gradually in this manner the characters of a capillary net-work, become dilated into wide sinuses, which communicate freely with the enlarged vessels of the muscular walls of the uterus. As the original capillary plexus occupied the entire thickness of hypertrophied decidua, the vascular sinuses into which it is thus converted are equally extensive. They commence at the inferior surface of the placenta, where it is in contact with the muscular walls of the uterus, and extend through its whole thickness, quite up to the surface of the foetal chorion.

As the maternal sinuses grow upward, the vascular tufts of the chorion grow downward, and extend also through the entire thickness of the placenta. At this period the development of the blood-vessels, both in the foetal and maternal portions of the placenta, becomes so extensive that all the other tissues which originally co-existed with

them fall into a retrograde condition, and disappear almost altogether. The villousities of the chorion are now hardly anything more than ramified and tortuous vascular loops; the remaining substance of the villus having been atrophied and absorbed in the excessive growth of the blood-vessels. The uterine follicles have at the same time lost all trace of their original structure, and have become mere vascular sinuses, into which the tufted fœtal blood-vessels project, with all their compound divisions and ramifications.

Finally, the walls of the fœtal blood-vessels having come into close contact with those of the maternal sinuses, become adherent to them and fuse with their substance, so that the two can no longer be separated without lacerating either the one or the other, owing to this secondary union and adhesion which has taken place between them.

The placenta, then, when perfectly formed, has the following structure:



At *a.a.* is seen the chorion receiving the umbilical vessels from the body of the fœtus through the umbilical cord, and sending out its compound and ramified vascular tufts into the substance of the placenta. At *b.b.* is the attached surface of the decidua, or uterine mucous membrane; and at *c.c.c.c.* are the orifices of uterine vessels which penetrate it from below. These vessels enter the placenta in an extremely oblique direction, though they are represented in the diagram, for the sake of distinctness, as nearly perpendicular. When they have once penetrated, however, the lower portion of the decidua, they immediately dilate into the placental sinuses, which extend through the en-

tire thickness of the organ, closely embracing all the ramifications of the foetal tufts.

The placenta, accordingly, when arrived at this stage of development, is composed essentially of nothing but blood-vessels. No other tissues enter into its structure; for all those which it originally contained have disappeared, excepting the blood-vessels of the foetus, entangled with and adherent to the blood-vessels of the mother.

There is, of course, no direct communication between the cavities of the maternal and foetal vessels. The blood of the foetus is always separated from the blood of the mother by a membrane which has resulted from the union and successive fusion of four different membranes, as already described, viz: first, the membrane of the foetal villus; secondly, that of the uterine follicle; thirdly, the wall of the foetal blood-vessels; and fourthly, the wall of the uterine sinus. This membrane, however, is extremely thin. If a villus from the foetal portion of the placenta be examined by transparency in the fresh condition, it will be seen that its blood-vessels are covered with a layer of homogeneous or finely granular material, 1-3500 of an inch in thickness, in which are imbedded small oval-shaped nuclei, similar to those seen at an earlier period in the villus of the chorion. This layer is all that intervenes between the foetal blood in the tufts of the chorion and the maternal blood in the placental sinuses. It is of enormous extent, owing to the extremely abundant branching and subdivision of the double vascular system which is thus formed. The vascular tufts, accordingly, in which the blood of the foetus circulates, are bathed everywhere in the placental sinuses with the blood of the mother; and the processes of endosmosis and exosmosis of exhalation and absorption may go on between the two with the greatest possible activity.

It is very easy to demonstrate the arrangement of the foetal tufts in the human placenta. They can be readily seen by the naked eye; and may be easily traced from their attachment at the under surface of the chorion to their termination near the uterine surface of the placenta. The anatomical disposition of the placental sinuses is, however, much more difficult of examination. During life, and while the placenta is still attached to the uterus, they are filled, of course, with the blood of the mother, and occupy fully one-half the entire mass of the placenta. But when the placenta is detached, the maternal vessels belonging to it are torn off at their necks, *c. c. c. c.*; and the sinuses, being then emptied of blood, collapse, and are apparently obliterated; and the foetal tufts falling together, and lying in contact with each other, appear to constitute the whole of the placental mass. The

existence of the placental sinuses, however, and their true extent, may be satisfactorily demonstrated in the following manner.

If we take the uterus of a woman who has died undelivered at the full term, or thereabouts, and open it in such a way as not to wound the placenta, this organ will be seen remaining attached to the uterine surface, with all its vascular connections complete. Let the fœtus now be removed by dividing the umbilical cord, and the uterus, with the placenta attached, placed under water with its internal surface uppermost. We then see the fœtal surface of the placenta formed by the chorion, and covered still by the thin and transparent amnion. The amnion should next be removed, which can readily be done by gently detaching it from the surface of the chorion. If the end of a blow-pipe be now introduced into one of the divided vessels of the muscular walls of the uterus, and air forced in by gentle insufflation, we can easily inflate, first the venous sinuses of the uterus itself, and next the deeper portions of the placenta; and lastly, the bubbles of air insinuate themselves everywhere between the fœtal tufts, and appear in the most superficial portions of the placenta, immediately underneath the transparent chorion. If the chorion be now divided at any point by an incision, passing merely through its own thickness, the air, which was confined beneath it in the placental sinuses, will escape, and rise in bubbles to the surface of the water. Such an experiment shows conclusively that the placental sinuses communicate freely with the uterine vessels, occupy the entire thickness of the placenta, and are equally extensive with the tufts of the fœtal chorion.

It is unnecessary to say that none of the air thus injected finds its way into the umbilical vessels.

I have now had the opportunity of doing this experiment with the results just described, on four different occasions since 1853. The first two cases occurred at Bellevue Hospital, in patients who had died of acute disease in the last stages of pregnancy. The third case was that of a woman who died undelivered, owing to hæmorrhage from placenta previa, at the end of the seventh month. The fourth was that of a woman who died of puerperal convulsions at the full term. The examinations were made at different times, in the presence of Dr. C. R. Gilman, Dr. Geo. T. Elliott, Dr. Henry B. Sands, Dr. F. J. Bumstead, Dr. Wm. H. Draper, Dr. Henry D. Noyes, Dr. T. C. Finnell, and Dr. J. W. S. Gouley, all of whom I believe were satisfied in every respect with the result of the experiment, and convinced of the existence of the placental sinuses, and of their free communication with the vessels of the uterus.

The method of injecting which I have now described has many advantages over that adopted by the Hunters and Prof. Weber.

In the first place, in order to demonstrate a doubtful anatomical point by means of injections, it is extremely desirable that the part to be injected should be freely exposed, and under the eye of the experimenter while the injection is going on. The progress of the operation can then be carefully watched, the amount of force used properly graduated, and the injection terminated as soon as the desired information has been obtained. Every one, I think, who is practically familiar with injections, will appreciate these advantages. But when the pregnant uterus is injected with wax, in the unopened condition, by the spermatic arteries or veins, the injection is made to a certain extent blindfold. We do not know when it is sufficiently complete, we cannot watch its successive stages, and any extravasations which occur may increase to any extent without being discovered. On this account, therefore, the injection by wax is both more difficult and less satisfactory than the one adopted in the above experiments.

In the second place, the injection of air in the manner above described is infinitely less liable than the other to mislead by producing extravasations. This, it will be remembered, is the great objection urged against the results obtained by Weber and the Hunters. It is said that their wax injections penetrated into the placenta by rupture of the uterine vessels, and the difference of opinion on this point shows that it is not easy to decide, from the appearance of the hardened specimen, whether the wax which has been injected be really contained in the cavity of vascular canals, or infiltrated between the tissues of the organs.

In the experiments detailed above, however, it will be recollected by those who were present, how very moderate was the force required to make the air pass from the uterus into the placenta, and even to penetrate quite up to the under surface of the chorion. The elasticity of the air, also, makes it much less likely to cause laceration of the vessels, than the liquid and incompressible wax. Furthermore, the body of the uterus being opened, in order to expose its internal surface, very many of its own vessels were necessarily divided; and the air, during its injection, bubbles out freely at numerous points from the orifices of divided uterine vessels, showing that no undue pressure can be exerted upon the walls of the vessels in its interior. In fact, no doubt can rest upon the mind after performing an injection in this manner, that the air does really follow the course of the vascular canals; and that when it penetrates into the placenta, and inflates its

entire thickness, it is still contained in natural cavities, continuous with those of the matured vessels.

Let us now see what evidence can be gathered of the anatomical arrangement just described, from the placenta as ordinarily expelled after parturition.

If the under surface of the placenta be examined after its detachment from the uterus, it will be found to present a lobulated appearance, owing to the existence of flattened rounded prominences, separated from each other by shallow furrows. Its surface is formed by a soft, whitish, opaque layer, about one-tenth of a line in thickness, which conceals from view the foetal tufts, and which is continuous, at the edges of the placenta, with the uterine decidua adhering to the external surface of the chorion. This layer is, indeed, a part of the decidua. It is the layer sometimes spoken of as "interposed" between the uterus and the placenta, though, in reality, a part of the placenta itself; and it is through it that the utero-placental vessels penetrate to expand into the placental sinuses.

These vessels penetrate, however, in such an extremely oblique direction, that their orifices may easily be overlooked; and it is on this account that it has been denied, by some authorities, that any openings are to be seen on the surface of the detached placenta, corresponding with the mouths of the lacerated uterine sinuses. These orifices, however, do exist, and may be seen by careful inspection. They are to be found more particularly in the furrows between the lobules of the placenta, and lead into the placental sinuses sometimes by a rather long and very oblique passage. They sometimes present themselves as rounded or oval openings, having but one well-defined crescentic edge, and blocked by the end of a foetal tuft, which sometimes even projects a little from the orifice, owing to the empty and collapsed condition of the maternal sinus. These openings are very numerous, also, in the terminal sinus which runs round the borders of the placenta, and from which the air easily penetrates into its central portions.

The placenta, accordingly, is a double organ, formed partly by the chorion and partly by the decidua; and consisting of maternal and foetal vessels, inextricably entangled and united with each other.

A Case of Ovariectomy successfully performed. By ROBERT NELSON, M. D., New York.

Madame L.—B—, 21 years of age, tall and genteel looking, sought my advice in June, 1854, believing herself to be in the eleventh month of her pregnancy, saying also, that such was the opinion

of her medical attendant and his consultants. On a superficial examination a slight fluctuation was perceptible, and a few replies to questions sufficed to show that her tumefaction was due to a right ovarian enlargement. This I told her, but she refused to believe me, left me dissatisfied, and took fresh advice, the same as before. A few months passed on, and she continued to enlarge, the abdomen augmented much in size, the lower part of the thorax rose high, by the tumor encroaching upon that cavity, crowding the diaphragm high up, and causing much dyspnœa. Although her appetite was good, so compressed was the stomach, that at last she eat only one meal a day, became lean and pale. It now became evident to herself that she could not live many months longer, and therefore she solicited me to do what I might think best for her, even if death should follow my means; for she became convinced in her own mind that it was better to risk an operation, even if followed by death in a few days, than to abstain from it, and linger on a few months of life with gradually increasing sufferings, to ultimately end in death. She made her arrangements accordingly, and submitted to the operation on the 11th of December.

Assisted by Dr. Huard, of Paris, and Dr. Yanney, of New York, and two other gentlemen, who requested to be present, we met at her lodging, a very small room, rather dark, damp, and cold, (46° Fahrenheit,) without means of warming it. Lying in bed, on her back, the abdomen well exposed, and chloroform administered, I commenced the incision a little above the umbilicus, and carried it down into the hair of the pubis. This cut brought the lower part of the tumor into view, but, proving of insufficient extent, was carried upward quite to the ensiform appendix, making an incision 22 inches long, by reason of the great convexity of the abdomen. It gaped only an inch wide, the recti muscles contracting rigidly, notwithstanding the anæsthetic state she was in. In front of the tumor, but high up, was seen a small portion of lean, gray omentum. With some force each hand was insinuated between the tumor and abdominal parietes, high into each hypochondrium, and strongly compressing it, while the assistants drew the recti apart, the mass rolled out of the abdomen, resembling a great graminivorous stomach. Broad flat veins meandered on its surface, traveling toward the broad ligament where they mostly congregated at its upper and lower borders, which were stretched six inches apart. A few gentle touches of the scalpel divided their peritoneal covering, and with a curved needle, a ligature of a single strong hempen thread buried in this cut was passed round them, and tied very tightly. The

same was done with the lower mesh. The intervening portion of the ligament held no vessels. As the tumor now lay out of the abdomen, by its weight drawing strongly on the broad ligament, it was easily severed near the ligatures from its attachment, so that not a teaspoonful of blood got into the abdominal cavity. The visceral and parietal peritonea were touched as little as possible. Thus far only a few minutes of time were required. The intestines gave no trouble, having become reduced in volume by long pressure, causing leanness and emptiness, and there was absence of flatus in them; so that now, the tumor being removed, the abdominal cavity looked like that of an eviscerated subject. The next step was to close this enormous cut, which, now that the distension was removed, was reduced to less than 14 inches long. Three long silver pins were first introduced, one near the navel, and one at equal distances above and below; between these, five common tinned pins of large size transfixed the remainder of the opening. Still the intervals gaped, and to close these eight interrupted sutures were inserted. Long adhesive straps were applied; and over the united line was placed a strip of old rag, dipped in blood obtained from the veins of the tumor. Over all, as a compress, were placed four napkins folded, and over these a large sponge, the whole secured by a five-tailed bandage. This great thickness of padding, for so it may be called, barely sufficed to bring the bandage on a level with the elevated ribs, so highly were they distorted.

The removal of the tumor occupied a few minutes only; but a considerable time was spent in closing the wound, for on the nice fitting of the edges I counted as one of the necessary means to ensure prompt adhesion, and escape inflammation. The two ligatures were brought out near the pubis.

The tumor weighed over 21 lbs., was composed within the sac of numerous cysts—three or four of them rather large, the others diminishing from the size of a small orange to that of a hazel nut, and a pea; the larger were filled with the usual thick, glutinous, chocolate-like fluid, and the small with clear, but ropy liquid. About 5 inches of the fallopian tube, with its fimbriæ, were removed with the tumor; it was pervious to a common probe for about 4 inches. The divided edges of the broad ligament retracted on the removed tumor about 3 inches apart and 8 inches long.

At 9 P. M., seven hours after the operation, complained of much dull pain, not in the soft abdomen, but under the hypochondriac cartilages, particularly the right one. This was due to the cessation of support these parts had lost by the absence of the tumor, and the

weight of the liver drawing heavily upon the suspensory ligament—a support that the thick padding imperfectly supplied. To take iv grs. of opium, and to have cold toast-water only.

12th, 7 A. M. Has had a comfortable night; skin moist and cool; pulse natural. *Evening.* Has had a good day. Having maintained the supine position, it was with difficulty that her urine could be passed without wetting the bed—an occurrence that her nurse, a most inefficient one, was heedless of; changed and dried her; skin is still cool, but the pulse increased in frequency. The only complaint she makes is the uneasy feeling in the hypochondria. To take iv grs. of opium.

13th. Has passed a very comfortable night, and complains only of the lassitude arising from her one position, and the dragging sensation in the hypochondria, which, however, is less than heretofore. Cool; pulse 90; no thirst. *Evening.* Good day; feels a sensation of flatus, and a desire to pass it, but cannot; has eaten nothing since the evening of the 10th, but is not hungry. To have dry bread and toast-water freely. To take a pill of iij grs. opium, iij of aloes, and iij of calomel; also an enema of tepid water.

14th. Was much disturbed during the greater part of the night by the riotous conduct of neighbors. Is a little feverish, tongue whitish, a little thirst. Repeat the enemata, the bowels not having acted. Gruel for breakfast. At 2 P. M. was rendered very hysterical by the act of a busybody, who called upon her to get some “explanations” regarding some previous tattlings. Bowels not yet moved. To take one ounce of castor oil; this operated three times during the night, followed by a very comfortable feeling.

15th. Felt well this morning; but at ten o'clock was thrown into violent hysterical convulsions, brought on by a quarrel with a visitor.

16th. Pretty well. 2 P. M. It is now the complete fifth day since the operation. Removed the dressings. The whole wound is united, excepting where the ligatures come out near the pubis, and here escapes a good deal of *liquor abdominalis*. The line of incision is not red; the skin of the abdomen is white and soft. She is very hollow, the belly is much below the level of the thorax, the ribs being still very prominent. Removed six of the thread sutures.

17th. Has had three motions produced by enemata taken at her own request. At 2 P. M. removed the dressings, which were saturated with *liquor abdominalis*. Took away two of the silver pins, which have caused suppuration in their course. The ribs are descending to place, but are still high, and the belly hollow, as if containing no intestines. Has eaten a quail for breakfast.

18th. Had a very good night, with long sleep. Catamenia appeared this morning, two days later than was calculated for at the time of the operation, to act as a critical discharge at the height of peritoneal inflammation should it be severe. Removed the third silver pin, which has caused a suppurated cavity containing a dessert-spoonful of thick white pus, and also removed all the tinned pins, none of which caused any suppuration. Less *liquor abdominalis* escapes to-day. Has a slight cough, from a cold taken, doubtless, at the time of dressing her in her very damp and cold room. To take ij grs. opium at bed time.

19. Feels very well, and is cheerful. Has more appetite than it is thought prudent to satisfy.

20th. Had epistaxis, to which she is subject, and which has relieved her cold.

21st. Very well, and very hungry.

23d. Was well this morning; but had another quarrel, growing out of something that a visitor had robbed her of, and which again produced hysterics. In other respects she is quite well. Catamenia have ceased; during its continuance had occasional epistaxis for three days. Escape of *liquor abdominalis* has quite ceased, probably owing to the peritoneum having adhered about the ligatures, enclosing them in a canal, and thus closing in the peritoneal cavity.

Jan. 2d. Has continued to progress favorably since the 23d December; is very well to-day, and got out of bed for the first time, twenty-two days since the operation.

5th. Was alarmed in the night by a noise taken for house-breaking; got up and rushed into the street to call for help, though the night was boisterous, cold and rainy; but has not suffered from so doing.

6th. The first ligature came off to-day; a length of six inches of it lay in the abdomen. She is up and about the house.

21st. Has continued quite well, with the exception of the retention of the second ligature, until this day. Her appetite is good, she lives generously, has filled out and fattened sensibly; the belly is much fuller, and the ribs having subsided gives her body a good appearance now. In some places the union line of the incision is not broader than the thickness of a half-dime. The sites of the interrupted sutures and of the tinned pins are fast disappearing, but those of the silver pins, which alone suppurated, are very marked. The length of the cicatrix has shortened greatly since the elevation of the belly and descent of the ribs.

22d. Passed the afternoon and evening at a friend's house.

23. Rode out to a place of recreation in the country; and after returning paid me a visit on foot.

As the number of these cases is not so great as to render the reporting of a few more a redundancy, I have ventured to give the foregoing one. It was the practice, in several of the cases operated on in England, to heat the apartment to avoid any injury that cold air might do to the peritoneum; but it will be remarked in this case that, though the patient's room was a cold one, no evil consequences followed. As regards the severity of such an operation upon the constitution, it will appear evident that the shock must have been slight indeed, since it was imperceptible, and delayed the usual occurrence of the catameniae only two days; that peritoneal inflammation was never manifest; and that during the night of the sixth day

—In somno mens grato agitur amore,
Fluctibus expergens veneris cruciatur amoris!

her idiosyncrasy—one that neither the disease nor the removal of one ovary subdued; which throws light on the origin, or remote cause of the ovarian hypertrophy in her case, and doubtless a temperament cause in the majority of cases.

This operation was performed 14th December, 1854; in a few weeks after she resumed her usual occupations, taking a great deal of exercise, and has continued in robust health until last heard from, a month ago.

New York, June 14th, 1858.

Puerperal Fever; an abstract of the Discussion at the Paris Academy of Medicine. Collated from the French Journals for the MONTHLY.

M. Trousseau.—The Academy has already been entertained for two or three sittings by a discussion upon puerperal fever. It has been spoken of as a sure thing, the existence of which no one denied. It is now time to reply to the question, "Is there such a thing as puerperal fever?" For my part, I do not believe there is. Not that I disbelieve in inflammations of great severity to which lying-in women are subject, but that I doubt the existence of a puerperal fever which is peculiar to women, and which cannot affect man.

Two years since, M. Lorain sustained before the Faculty an excellent thesis, entitled *Puerperal fever in the recently delivered female, the fetus, and the new-born child*. I am sorry that M. Lorain did not add, to make it more complete, "and in males who are present in hospitals in which lying-in women are received." In fact, in all patients placed

under such circumstances, the lesions exhibit an appearance, a particular physiognomy which more or less resembles that found in women attacked with puerperal fever.

Let us not, however, anticipate, but let us first examine what takes place in the *fœtus* and the new-born child during the prevalence of puerperal fever.

Every writer and every observer, who has studied this subject, agree that during an epidemic of puerperal fever the mortality of new-born children is considerably increased. MM. Charlier and Lorain, who gave in their thesis a very complete account of the last epidemic at the *Maternité*, have proved this fact, which had already been pointed out, but they have in addition shown that infants die of lesions analogous or similar to those observed in women victims of puerperal fever, of phlebitis, of peritonitis, of pleurisies, of pneumonias, and other visceral inflammations, which, independently of metritis, were very common during the epidemic of 1856, at the *Maternité*.

It is not only the infant born at full term and detached from the mother who thus suffers, but the infant born before the full term can enter the world with a peritonitis, a double pleurisy, contracted in the womb of his mother who is attacked with the symptoms of puerperal fever. I should instance still another fact, that women recently delivered, and who are received into other hospitals from the *Hopital des Cliniques*, or the *Maternité*, almost always come with children afflicted by severe ophthalmias, erysipelas, umbilical phlebitis, inflammation of the *vena portæ*, or other lesions generally mortal, and analogous to those which carry off the recently delivered in the special hospitals. Finally, and what is remarkable, children who are brought in to these women to be nursed, are frequently exposed to this deplorable influence.

Now, what takes place with the adult and male individuals who are treated in a hospital during the prevalence of puerperal fever? Ask M. Nelaton, M. Laugier, or M. Jobert what happens under these circumstances to their patients who have been operated upon, or their wounded? Ask them if it is then prudent or opportune to operate with the knife or bistoury? Ask them if they cannot surely foretell the existence of puerperal fever in an adjoining ward by observing the wounds take on a bad aspect, or being covered with hospital gangrene and their patients attacked by serious symptoms, which accompany purulent absorption, multiple abscesses, and visceral phlegmasias?

Is it necessary that a wound should, so to say, open the door to contagion as in the preceding cases? Assuredly not, as the following will prove. In 1846, M. P. Dubois closed his clinic, it being invaded

by a formidable epidemic of puerperal fever. The lying-in women were scattered through the other hospitals of Paris. The wards of the hospital of M. Dubois were opened to ordinary diseases, under the charge of M. Pidoux. Soon he was surprised to see that every disease assumed promptly a singular physiognomy, which became aggravated notwithstanding the best treatment, and soon ended in death. M. Pidoux discovered in these symptoms the malignant influence of puerperal fever, which still remained in the wards after the departure of the lying-in women.

It is the same with surgical diseases where there is no wound—tumors, for example. Surgeons can tell you that in the wards adjoining those where women are delivered in which puerperal fever is present, it is not rare to see, even in patients neither wounded nor operated upon, symptoms arise such as want of appetite, diarrhœa, followed by general symptoms of considerable seriousness.

Such are the facts which seem to me to sustain the opinion I have enunciated, and which at first appeared paradoxical to those who admit, without reserve, the existence of puerperal fever.

Now that you have heard my profession of faith relative to puerperal fever, let me bring proofs of it. I shall not retrace the history of this disease; this has been already done most admirably by M. Depaul; I shall only give the outline of some of the most prominent features, in order to show more clearly that they can be found out of the puerperal influence.

In one form, which I shall call purulent, with M. Hervez de Chégoin, we see phlebitis of the uterine sinus, of the ovarian veins, of the hypogastric veins, of the veins of the leg, abscesses in the viscera, collections of pus in the serous membranes, &c.

In the acute putrid form, which had been described by M. Danyau and others before M. Hervez de Chégoin, there is a series of severe ataxo-adynamic phenomena, which simulate the symptoms of a fatal intoxication.

Finally, there is a third form, which I shall call *nervus puerperal typhus*, in which disorders of innervation are alone remarked, which are present from the first, taking immediately a most serious character, which forms the most dangerous and the most fatal of the forms of puerperal fever.

Let us see, now, if these symptoms are present in other conditions than in the puerperal state.

M. Lorain was the first to demonstrate conclusively the existence of puerperal fever in the fœtus. He has reported eight cases of

the fœtus dead in the mother's womb from purulent puerperal fever, with pus in the tissues of the viscera and in the serous membranes; false, fibrinous membranes upon the pleura, upon the peritoneum, and agglutination of the intestines.

I had, as early as 1842, called the attention of my students, at the Necker hospital, to the erysipelas and phlebitis of the new-born, and was in the habit of designating their appearance under the name of puerperal fever of the new-born. This idea has been developed both by M. Delpech and M. Bouchut, but no one has furnished such authentic proofs as M. Lorain, for establishing the remarkable identity of the puerperal symptoms in the female, the fœtus, and the new-born.

But it is said the fœtus, and even the infant just born, are still parts of the mother, and appertain to her organization, as a graft belongs to the plant upon which it is grafted. That is true, but still I will add that infants have been attacked with puerperal symptoms in the maternal womb, when the mother had apparently nothing—that is to say, she was, as yet, only under the latent influence of a diathesis which she could overcome, and which, in fact, she frequently subdues.

Let us add still more. M. Tarnier reports that in the midst of the severe epidemic of 1856 a woman, in most excellent health, came to the hospital. At the commencement of labor, before the neck was fully dilated, before any tearing, or ulceration, or wound of the tissue of the uterus could have taken place, she suddenly had a terrible chill; it was the signal of a puerperal fever, which carried her off a few hours afterward.

M. Tarnier also reports that he has seen young women, studying as *sage femmes*, die of puerperal fever, taken during their menstrual period. It will probably be said that menstruation is a kind of puerperal condition, and that the epithelial exfoliation which then takes place from the mucous surface of the uterus, offers some analogy to the placental exfoliation. I reply to this objection by the following fact: A young woman, who had menstruated the three or four first days of May, was taken sick upon the 11th, after she had finished menstruating several days, and died the 13th or 14th from a severe puerperal fever. It is true, however, (which detracts much from the weight of my observation,) that, although this woman continued at her occupation, she complained of feeling unwell and of loss of appetite for the five or six days which followed the flow of the menses, so that in this case even the morbid infection might have been contracted at the time of the catamenial exfoliation.

Let us now see what happens to the wounded and those operated upon in hospitals in which puerperal fever is present. In 1826, M. Velpeau, in an excellent memoir, pointed out the frequency of symptoms called purulent absorption, among the patients in the wards of M. Antoine Dubois, at the Clinique Hospital, which then received, as now, a great number of lying-in women. A little later, Dance published some similar facts. In 1838, M. Tessier wrote a valuable work upon purulent infection, in which he established a similarity between certain symptoms observed in those who had been subjected to operations and the phenomena of the female attacked with puerperal fever. M. Velpeau, M. Dance, M. Tessier, all describe phlebitis, purulent collections in the muscles, in the viscera, in the serous cavities, &c., in these cases.

In hospitals, where puerperal fever reigns, putrid infection destroys quite as many among the wounded as purulent infection.

As to nervous typhus, that which is observed in women in the puerperal condition, differs in no respect from that which is met in patients wounded, following alcoholic excesses, or that which succeeds the great traumatic injuries upon the field of battle. There is the same physiognomy following the traumatic as the puerperal nervous typhus.

These are the facts, and I believe I have sufficiently demonstrated that the serious lesions observed in women recently delivered are also observed in individuals beyond the influence of the puerperal condition. It is not, therefore, in this condition that the cause of these symptoms should be sought; it is necessary to seek for it in something more general, which I shall strive to define.

I do not, however, wish the Academy to suppose that I am not a specialist and the enemy of the local cause of this disease. [M. Trousseau then reviewed the opinions of those who had preceded him in the discussion upon this point, as well as those whose opinions had already been stated in their writings, such as MM. Velpeau, Dance, Behier, and Dumont-Pallier, and then continued thus.]

Is it true, gentlemen, that pus can be absorbed? I ask the most rigid micrographist who has mathematically ascertained the dimensions of the pus globule, and the capacity of the capillary vessels. All are of accord in replying that the globule, which is the essential element of pus, cannot be absorbed; it cannot circulate in the capillary ramifications.

Admitting that pus can be absorbed, can the following facts be explained by absorption?

A woman dies after two or three days' illness, from puerperal fever, and five or six quarts of pus are found in the pleuræ or in the peritoneum. Can it be said that this pus was taken up from the uterine cavity and poured by the vessels into the serous cavities? And the fœtus that dies from puerperal symptoms, with visceral abscesses and purulent collections in the pleuræ, or in the peritoneum; where does all this pus come from? There is no wounded surface in a state of suppuration, from which it could have been absorbed? Cannot I say the same of women who are attacked with puerperal fever during the menstrual period, or even when they are not in that condition? Cannot I say the same of those who, entering the surgical service of an hospital, adjoining the lying-in wards, for the purpose of being operated upon for some cancerous tumor without being wounded, nevertheless suffer from the epidemic influence, and at the autopsy present all the lesions of purulent infection already described?

It is necessary, then, to seek elsewhere than in absorption for the origin, the source of that pus which is poured in such profusion into the organism.

In truth, gentlemen, I have always admired the long-suffering of the surgeons of Paris. It is a fact that, in the hospitals of large cities, in Paris particularly, the mortality succeeding great operations exceeds that of the hospitals of small cities, and specially that of the country. Is it because the surgeons of the country and small cities are more skilful than those of Paris? Certainly not. Is the cause of this difference, then, in traumatism? Not at all, for this is the same everywhere, in the small city as in the large city. Wherein is then this cause? We shall soon see.

[In this connection M. Trousseau discussed the opinions of M. Beau, who regards puerperal fever as symptomatic of a phlegmasia, which is in its turn the result of an inflammatory diathesis. M. Beau's views will be found in the preceding number of the *Monthly*. M. Trousseau thinks that M. Beau is wrong in not distinguishing puerperal symptoms from puerperal fever properly called, and he accuses all those who believe in the local character of the disease, with making the same mistake.]

The difference between the symptoms, simply puerperal and the fever called puerperal, is explained by a special character (*spécificité*), which does not affect the first, but which influences puerperal fever.

Take the pus of variola and of ecthyma, ask an able chemist to analyze them, to submit them to every reagent, and then ask him if he finds any difference between them. Strive to find, if you can, by the

most severe analysis, and the most delicate reagents, any difference between the poison of the viper and gum water. You will find absolutely no distinctive characteristic in either. But there is another reagent more subtle, and more powerful than the chemical reagents, the *living reagent*, which will show you with a marvellous distinctness the difference between the pns of variola and ecthyma; the poison of the viper and gum water. Is there not something special in this? would you not call this *specific*?

But tell me, you who are specialists, how do you distinguish a severe simple peritonitis in a lying-in woman from a special peritonitis, from a peritonitis of puerperal fever? Although I cannot tell the difference, or establish the distinction, does it follow from this that none exist? Recall what I have just said in relation to the chemist, who is unable to distinguish the pus of variola from that of ecthyma, the poison of the viper from that of gum water. You, who do not admire the nosological divisions, and who reject the idea of a morbid specialty, do you confound, notwithstanding these common characteristics, a severe common colitis with an epidemic colitis, with contagious dysentery; simple enteritis with dothinerteritis; simple bronchitis with whooping cough? Apply these same principles, this same prudence, to the distinction of puerperal fever, and puerperal symptoms.

What is the difference between special diseases and those which are not? Van Swieten will tell you. [M. Trousseau quoted a passage from the Commentaries of Boerhaave.] There exists then in diseases a kind of *ferment*, a *morbific matter*, the nature of which is only known by its effects; but which once developed in the human body, extends its sad influence throughout, and soon assimilates the whole economy to its own substance, as the yeast of beer thrown into a sweetened liquid is not long in transforming, in assimilating the whole of it to its own substance, however great may be the quantity of the liquid.

The organism receives some virus, which is a truth requiring no demonstration; but it fabricates some, also, which all do not admit.

The morbid matter, the virus once developed or brought into the economy, germinates, is developed, and produces apparent effects; it finds a convenient place of action. But there is no more favorable location for the germination of morbid ferments than that of wounds. Poison produced in a point of the organism increases, propagates itself, and, as I have just said, soon assimilates the whole economy to its own substance. It is in this way that an atom of pus or of the virus of variola in the mass of the blood is sufficient to produce puru-

lent infection, or for causing the marked manifestations of variola to break out.

I believe, then, that there is also in puerperal fever something special, which constitutes the character and the foundation of the disease. This something belongs to the lying-in female, who undoubtedly is in a grand *morbid opportunity*, in a remarkable *pathological aptitude*.

At the time of menstruation, do we not see various phenomena of the intelligence appear; various difficulties of innervation; modifications of the mammary apparatus, which certainly indicate a general cause? During gestation, do we not observe changes in the blood, in the urine, in most of the secretions, and also disturbances in nutrition, owing to the same cause?

Finally, at the time of parturition, are not a series of phenomena produced which are well known, and which show the action of a general cause—the same doubtless as those, the influence of which we observe during menstruation and gestation, but now manifested by more observable effects, by characters more distinct? All these phenomena are the various results of what I shall call the *génésique fever*.

Is it astonishing, then, that a female in the conditions I have just described, should be subjected more easily to the influence of certain ferments, certain pathological poisons? This is the *puerperal diathesis*; it is a great opportunity for contracting disease—nothing more.

Considering the subject of treatment, M. Trousseau remarked, that he also had had his successes and his hours of illusion; but that, since he had learned to distinguish puerperal symptoms, properly called, he found he was not any more successful than others against true puerperal fever. He did not believe that the best ventilation in the world would influence an epidemic. Evidence of this is seen in camps in the open air. Nor did he believe in uterine and vaginal injection, of which so much is said. In puerperal fever, as in wounds, the infection is owing to something else than the contact of pus with the traumatic surface, and its longer or shorter sojourn upon the naked tissue. There is something of which we are ignorant, and for which the most assiduous washings and the greatest attention to cleanliness is of no account. Nor did he believe in the treatment by sulphate of quinia. As a preventive measure, he had no confidence in it; for in those cases in which it has had an apparent success, how is it known that they were cases of true puerperal fever? for it is well understood that, in seasons of an epidemic, all cases take on the character of the reigning disease. The capriciousness of an epidemic has been often remarked, invading one ward and leaving another untouched; affecting all fe-

males who enter a hospital to-day, and suddenly disappearing to-morrow. This, if I recollect aright, was the case with the wards under the care of M. Piedagnel, they being one of the privileged class; so that, notwithstanding his great authority, I am still doubtful as to the preventive action of sulphate of quinia.

It is the same with the curative effects of this salt. The cases reported cured by M. Beau, are those where the peritonitis was limited to the sub-umbilical region. I can readily believe this; but I also believe that a peritonitis so simple, so limited, might be cured by the use of emollients or local anti-phlogistics, and even by expectation alone. The mistake is in confounding puerperal fever with simple puerperal symptoms. We also cure these latter, and without the aid of quinine.

I close, declaring, as at the beginning of my remarks, that puerperal fever is a specific disease which attacks lying-in women, new-born children, the fetus in the maternal womb, women not in the puerperal state, men even, wounded or having a limb amputated, and other patients also, who are in conditions of puerperality. I add, that the treatment of puerperal fever, both prophylactic and curative, is yet to be discovered.

Influence of Manufactories of Chemicals on Vegetation and Public Hygiene. From the Journal de Chimie.

A commission was appointed by the Belgian government, August 30, 1854, to investigate the effects of certain manufactories of chemicals on the operatives employed, as well as on the vegetation in the neighborhood. This commission was divided into two sub-committees, in order to hasten, as much as possible, the solution of the numerous questions that were involved. One of these, composed of two chemists, was specially charged with the duty of visiting the shops, the minute examination of the apparatus employed, and the study of means suitable for lessening the effect of acid emanations; the other, formed of botanists and agriculturists, directed its attention to the condition of vegetation in fields and forests adjoining manufactories. These sub-committees have furnished a report which is of general interest.

The committee of chemists, in order to make an exact account of the influence of each process, made analyses of the different products, at various stages in the process of manufacture, and often analyzed the materials first employed. A thorough study was made of the nature and quantity of gases escaping from the chimneys of the manufactories, which have been considered as most potent causes of the unhealthiness

of the neighborhoods. Their work was confined exclusively to establishments in which sulphuric acid, nitric acid, sulphate of soda, chlorhydric acid, soda, chloride of lime, and copperas (sulphate of iron) were made. Such of their results as possess especial interest, have been selected for our pages.

Manufactories of Sulphuric Acid.—The manufacture of sulphuric acid, despite the perfections which science has endeavored to introduce, can rightly be placed among those of an unhealthy character. The inconveniences which arise from its manufacture depend principally on the difficulty of completely condensing the gases, which, by their reaction in the leaden chambers, give rise to the sulphuric acid. Among these gases are two which, in consequence of the manner the process is carried on, can escape from the chambers—sulphurous acid and hyponitric acid—both irrespirable, and especially injurious to vegetation. It was interesting, then, to examine what was the influence exercised by these gases in the manufactories where they were produced. It was first necessary to determine their real presence and proportion in the emanations from the manufactories. The commission made this determination by two different methods—from data furnished them by the workmen employed in the manufactories, and by direct analyses. It will be understood that, if we sum up on the one hand the quantity of sulphuric acid annually made in a manufactory, and on the other hand the quantity of sulphur burned in the same establishment, we can, by subtracting from the latter the amount which exists in the sulphuric acid, obtain the quantity of sulphur burned which has *not* been condensed as sulphuric acid, and which would be communicated to the atmosphere as *sulphurous acid*. There is, however—and this circumstance has not been overlooked by the commission—a source of error in this method of calculation, which rests upon the fact that a portion of sulphur, very small generally, may escape as sulphuric acid. The commission, therefore, considered it necessary to analyze the gases directly at their escape from the chambers, and thus found the results obtained in this way slightly different from the first results.

But here a difficulty occurred, the extent of which chemists will appreciate. The commission was only able, at these places—that is, the opening of the chimneys—to make qualitative examinations for the determination of the true nature of these gases; their quantity had to be determined in the laboratory in specimens transported thither in flasks. Thus they determined readily the presence of nitrous compounds at the opening of the chimney, which they could never detect in the gaseous mixture analyzed in the laboratory, in consequence,

doubtless, of the formation of a small quantity of sulphuric acid. This circumstance detracts somewhat from the interest of the results of the commission, in that it only furnished the absolute per centage approximately of the sulphurous acid. The result of the examination of the gases escaping from the chimney was the determination of an average per centage, daily, of Sulphurous Acid 0.955, Oxygen 14.961, and Nitrogen 84.08. For an average manufacture of 1,381,609 kilogrammes of sulphuric acid produced, there was a daily loss of 225 cubic metres of sulphurous acid gas.

Manufacture of Soda.—The inconveniences arising from the manufacture of this article are greater than in the case of sulphuric acid, since the difficulty is greater of condensing the gases at their exit from the chimney. The smoke from the latter usually contains the products of the combustion of pit-coal, a notable quantity of sulphuric acid, and a much larger quantity of chlorhydric acid. In four manufactories there was an average daily loss of 613 cubic metres of chlorhydric acid. The amount varies with the method of condensation employed by the manufacturer. The commission state that high chimneys ensure the removal of noxious emanations, which, although diluted with large quantities of air, *still* descend to the ground, and in wet weather this takes place very rapidly, so that such chimneys only ensure a greater radius for the deleterious emanations. They advise that the Government should not suffer high chimneys to be erected, unless they were supplied with apparatus for condensation.

The second part of the report of the commission consists of an examination of *the alterations in vegetation produced by these emanations*. In a general way, attention was paid to their action, according to the direction of the winds, the humidity of the atmosphere, rains, the topographic configuration of the soil with its reliefs; and they concluded, as would have been easily predicted, that, other things being equal, the effects were more sensible, in the most usual direction of the wind, under the influence of foggy weather, or rain, and when the smoke, either from the disposition of the surface or the direction of the wind, was brought into close contact with the surface. And wherever the effect produced by the smoke was manifest, it was possible to demonstrate by re-agents the presence of the chemical agents to which it was due. These were ordinarily chlorhydric or sulphuric acids. On the other hand, it was not possible to detect the presence of these acids on vegetation of the same species, when *no* signs of alteration were noticed.

But dare we conclude from these observations that no effect, in fact,

is produced, except where it is possible to determine the presence of a chemical agent? The commission have not gone as far as to insist on that. They have, however, given us the relative sensibility to the influence of these acids, of a number of plants. This number comprises thirty-four trees and shrubs. The most sensitive of all was the hornbeam, (*Carpinus Betulus*,) and the least so, the raspberry, the spireas, and the alder.

The conclusions of the commission are summed up as follows: 1st. Acid emanations, which escape from manufactories of chemicals, are capable of injuring the growth of certain plants; 2d. Nevertheless, the effects are produced in such an unequal way upon different kinds of ligneous and herbaceous plants, as that certain species appear to resist the harmful influence of the acid gases very well, while others are injured by the same, but to various extents; 3d. Among the last some cease to show any sign of alteration, even at a slight distance from the manufactories, whilst the alteration of others is effected at great distances, but always within certain limits; 4th. The radius of the injurious influence of such acid gases depends on several circumstances essentially different, but which cannot be absolutely determined; although in each given case they can be determined practically by observing at what distance vegetables, which are readily injured by the emanations, cease to present such special alterations as could be ascribed to their action; 5th. The radius of injurious influence, determined in this way, differs very much, not only in different establishments, but even in different directions from the same; and it was always greater in the direction of the dominant winds, whilst in the direction of winds less frequent it was always small and inconsiderable; 6th. In the direction of the dominant winds, the influence did not extend beyond 2,000 metres as a maximum, nor below 600 metres as a minimum.

Influence of such Manufactories on Population.—From the data collected by the government for 5 years, with reference to births and deaths, it appears that the means were favorable to the increase of population, as well in the districts where the manufactories were established as in those adjoining. Thus, from 1839 to 1843 the ratio of deaths was 1 in 66, and from 1844 to 1848, 1 in 58. It is remarkable that no case of cholera had appeared in these manufacturing districts, and that, with the exception of Floreffe, all were exempted from the typhoid epidemics which have prevailed in the province since 1843. During the epidemic of Floreffe, which raged in the hamlet of Buzet, where there were 75 sick, only one of the laborers who worked in a chemical

manufactory was attacked by the disease, and he had taken a long journey two days before.

A report of Dr. Cambrelin, president of the Medical Commission of the Province of Namur, states, that the emanations from the chemical manufactories of the valley of the Sambre do not give rise to any peculiar affection, and that the diseases of the chest are not now as frequent as formerly in the districts where the manufactories exist. The general health of the neighboring population remains the same as in the past—even that of the laborers occupied in the manufactories; and if they are ever attacked at the beginning of their apprenticeship with laryngitis, bronchitis, or difficulty in respiration, custom soon causes these indispositions to disappear, without any recurrence. The same observations are made as to the horses employed in the establishments.

Thus, contrary to expectation, the ratio of mortality is diminished in the midst of these manufactories of chemicals; but should this diminution be attributed to the direct influence which these manufactories have on the health of the workmen and the population? No one would dare to assert this. These manufactories have contributed, in one sense, to this happy result, by extending comfort around them, and offering regular employment, with suitable wages, to a portion of the population. In any point of view, and whatever part we may wish to ascribe to them in the way of direct influence, none of the facts will authorize us to conclude that these manufactories have exercised an *injurious* influence on health, and that they are not a cause of prosperity for the countries in which they have been established. L. H. S.

On the Change of Color in Venous Blood of the Glandular Organs, in repose and in action. By M. CLAUDE BERNARD, Journal de Chimie et de Pharmacie.

It has been admitted, ever since the discovery of the circulation, that the blood of the arteries is red, and that of the veins black, with the exception that the opposite holds good in the case of the pulmonary veins and arteries. This characteristic furnished Bichat the basis of his great division of the circulation, which was afterwards adopted by all anatomists—a vascular system with red blood, which carried the blood from the lungs throughout the whole body, and a vascular system with black blood, which returned the blood, from all parts of the body, to the lungs. The investigations and observations made by Claude Bernard within the last few years, and the results of which he

communicated to the Institute, January 25th, 1858, show that this proposition cannot be accepted as absolutely true.

This skillful observer, in fact, determined, in a large number of vivisections, that the blood contained in the veins is sometimes black and sometimes red, and that when it presents the latter appearance it remains black in the vena cava inferior which receives the blood of the vein.

The fact being once established, the cause of the difference of color can be well determined, and it is no other than the state of activity or repose of the kidney in its secretion of urine. He has demonstrated, by the most delicate experiments, in his course at the College of France, that, when the urine flows from the kidney into the ureter, which transmits it to the bladder, or when it has just been formed, the blood contained in the veins is red, and that it again becomes black when the flow of the urine ceases.

The same experiment having been made on the sub-maxillary gland of a dog, furnished the same results. The flow of saliva through the proper conduit of this gland, and the presence of red blood in the afferent vein, were two phenomena exactly coincident, just as the absence of saliva and a black discoloration of the blood in the same vein. Some analogous experiments were made on the parotid gland and the glands of the abdominal portion of the intestinal tube, which gave similar results; but, adds the author, with the habitual strictures he exhibits in his conclusions, the study of the subject cannot be complete until such experimental researches have been pursued on every gland separately.

It results from the facts contained in the paper of Claude Bernard, that if, in a physiological point of view, we should preserve the qualification of red blood as peculiar to arterial blood, (which is, properly speaking, nothing but the venous blood of the lungs,) that of *black* blood can only be maintained in a general way for the venous blood. From some other investigations of the same author, it appears that some physical and chemical modifications correspond with the different states of coloration, and should be taken into consideration in analyses of blood, whose composition is not the same in a state of activity and of repose in the same organ.

The last point of view is not only applicable to the glands, but it should embrace all the organs of the body, whose venous blood should be studied both in a state of repose and of functional activity. It is worthy of notice, that although the blood flows red from glands in a state of activity, on the other hand it flows of a very black color, and

with different physical qualities, from a muscle which is contracting. This latter fact was specially announced by Mons. Brachet, of Lyon, (*Physiologie Elémentaire de l'Homme*), in 1840, who demanded the right of priority of discovery, in a note communicated to the Academy of Sciences, in the session which followed the reading of Claude Bernard's paper. But Brachet claimed this fact as general, that "always and everywhere, the blood loses its red color in traversing organs, and loses a certain quantity of hematosine; *but the loss is larger when the organs are in a state of activity; that the blood becomes blacker even when the organic act removes nothing from it, as when a muscle contracts.*" But if the opinions of the two authors are the same with reference to the blood obtained from muscles in a state of activity, they are diametrically opposed as concerned glandular blood, and it is to be regretted that M. Brachet, in his communication, was not explicit on this divergence.

Claude Bernard, in his experiments on the sub-maxillary gland, has been enabled, by means of electric excitation, to provoke the activity of the organ, as it were at will, from which was produced the secretion of saliva and the red coloration of the blood contained in the vein; and this fact suggested to him the remark with which he terminates his paper: "All the modifications produced in blood by the functional activity of the organs, are always determined by the nervous system. Consequently it is necessary, at this point of contact of the organic tissues and the blood, to investigate what may be the special rôle of the nervous system in the physico-chemical phenomena of life." And he adds that the development of the facts which belong to this point in general physiology, will be the subject of a communication, which we shall not fail to present to our readers. L. H. S.

The Influence of Light on Animals. Translated from a paper by Mons. T. BECLARD.

During the last four years we have been performing, in the laboratory of the Faculty of Medicine, a series of experiments relating to the influence of ordinary white light, and of the different colored rays of the spectrum on the principal functions of nutrition. The object of this article is to present, by anticipation and in a concise form, some of the most important results of these experiments.

I.—The nutrition and development of animals not possessed of lungs or bronchia, who respire through the skin, appear to suffer very remarkable modifications under the influence of different colored rays of

the spectrum. The eggs of the *musca carnaria*, taken in the same group and placed at the same time under bell-glasses of different colors, give forth maggots; but if, at the end of four or five days, the maggots, under the bell-glasses, be compared, their development will be found to differ very much. Those most fully developed will be found under the violet or blue, and those least developed under the green. The different colored rays can be grouped, as regards their action on the development of maggots, thus: violet, blue, red, yellow, white, green. Between the development of those under the violet, and those under the green ray, there was a difference of one-third in general size and length.

II.—This first result induced us to examine the function which explains best, if I may so say, the quantity of organic metamorphoses—I mean the respiration, whose products can be received and estimated.

A long series of experiments upon birds has shown us that the quantity of carbonic acid produced, in a given time, through respiration, is not sensibly modified by the different colored bell-glasses under which they had been placed. The same was the result with the lower mammalia, as mice. We must, however, remark that the skin, in birds and mammalia, is covered with feathers or hairs, and that the light does not strike its surface. But we know, from the researches of Régnault and Reizet, that the change of gases (endosmose, &c.,) which takes place at the surface of these animals, is very slight.

III.—When we examine the influence of the different colored rays of the spectrum on frogs, who have a naked skin, and whose cutaneous respiration is energetic, (it equals and often surpasses the pulmonary respiration,) we can obtain remarkable facts. Our experiments here were only made with the green and the red ray. In the green ray the same weight of frogs produced, in the same time, a larger quantity of carbonic acid than in the red ray. The difference may be more than a half, generally a third, or a quarter more.

IV.—The skin of the animal (very likely the color of the skin) appeared to have a determining influence on the preceding results. For example, if a certain number of frogs were placed under a green glass, and a like number of the same weight were placed under a red glass, and the quantity of carbonic acid produced weighed at the end of 24 or 48 hours, the excess was in favor of the frogs placed under the green glass, as we have just mentioned. Immediately take the skins off the frogs and replace them in the same conditions, the experiment will change; the quantity of acid produced by the skinned frogs will be larger in the red ray than in the green.

V.—The influence of the colored rays of the spectrum upon the proportion of carbonic acid exhaled, in a given time, by a living animal, continues for some time with the dead animal, (muscular respiration,) and ceases as soon as putrefaction begins; that is to say, after the disappearance of cadaveric rigidity. Butcher's meat taken on the day following the death of the animal, or the next day, (then the cadaveric rigidity has ceased,) furnishes always, for an equal number of pounds, the same quantity of carbonic acid when its fragments are placed under the different colored rays.

VI.—A small number of experiments tried on the cutaneous exhalation of aqueous vapor, show that in the dark, (at the same temperature,) frogs lose, through evaporation, an amount of water one-half or one-third less than when exposed to ordinary white light. In the violet ray, the quantity of water lost by the animal in a given time is precisely the same as in white light.

L. H. S.

Inhalation of Carbonic Acid as an Anæsthetic. From the French of Dr. OZANAM.

The effects of carbonic acid resemble those of ether, according to the author, but are more fugitive; and while it is necessary in the case of ether to interrupt the inhalations after short intervals, an opposite procedure is required for carbonic acid.

a. As long as one wishes the sleep to be prolonged, the inhalations must be continued.

b. These can be prolonged ten, twenty, thirty minutes and more, without danger to life.

c. When the inhalations are stopped, the waking is almost always immediate.

The experiments of Ozanam and Faure have never resulted in death. When death does take place, it is slow, progressive, and one can predict for some time in advance the moment of its arrival, by considering, as Faure has done, the condition of the heart and the pupils. The following experiment, related by Ozanam, is most interesting.

I had prepared by Mons. Fontaine a gas bag containing about 100 litres of carbonic acid, being resolved to prolong the anæsthesia as far as possible. The animal was put to sleep in three minutes, without convulsions, and remained on its side in a quiet sleep without being held. The inhalations were continued for 87 minutes, and the apparatus was then withdrawn; full sleep lasted 5 minutes more; towards the

tenth minute the paws began to be agitated; at the fifteenth the animal arose. One hundred and two minutes were thus consumed in the experiment—a time much longer than is required by the longest operations.

We believe that Faure and Ozanam purpose the use of *asphyxiated anæsthesia*, or *anæsthesia produced by carbonic acid*, for man. Faure and Ozanam say, that they have respired the gas, if not to the point of producing sleep, at least until they felt the first effects. Its taste is slightly piquant, about as pleasant as that of ether, and it is an excitor of the saliva. Ozanam says that the ethers, chloroform, and carbonic oxide determine anæsthesia by robbing the arterial blood of its oxygen, so as to produce carbonic acid, and thus making the blood venous. Carbonic acid itself does not decompose the blood; it removes no vital principle from it; but contributes progressively, and so that it can be graduated at will, the necessary quantity of carbon to determine the insensibility.

L. H. S.

A Case of Labor in which the Fetus presented by the nucha and back of the head—demanding craniotomy. By CHARLES A. BUDD, M. D., Teacher of Obstetrics in the New York Preparatory School of Medicine, &c., &c.

The following extraordinary case is one which, both from its rarity and peculiarity, I am inclined to regard as worthy of record.

Mrs. B—, a short, thick-set woman, was confined on the evening of March 29th, 1858, with her third child. She had been in labor nearly *forty hours*, when she was seen by Dr. Geo. T. Hough, of this city, who, recognizing difficulty, desired that I should be sent for. It seems that Dr. H. had succeeded a practitioner who, having become alarmed, had received his fee and left. The woman was beginning to show unmistakable evidences of exhaustion, and the necessity of immediate interference was very evident. Upon making an examination I could not satisfactorily detect the position, although I could distinctly recognize—high up—the head, and *what appeared to me* to be one of the mastoid processes; the os uteri was not fully dilated, nor did the presenting part at all impinge upon it during a pain. The sounds of the fetal heart were not audible, both the doctor and myself having failed to detect them after several trials.

After submitting the woman to the influence of chloroform, I attempted to apply the forceps, but was foiled in whatever way I endeavored to pass the blades; and then found, upon a closer and more careful inspec-

tion, that the *nucha* and *back of the head* were offering, and that which I had mistaken for one of the mastoid processes was, in fact, the seventh cervical vertebra or vertebra prominens. It seems that the woman had left lateral obliquity of the uterus, and the vertex, instead of engaging in the pelvic cavity, had over-ridden the brim, so that the entire vault of the cranium was impinging upon the right ilium, and the uterine contractions had expended their power in endeavoring to force the neck of the child through the pelvic inlet. The walls of the uterus were so closely contracted around the body of the child as to preclude the idea of either version, or rectifying the position with the vectis, and my only alternative was to perforate. This I succeeded in accomplishing at a point nearly midway between the posterior fontanelle, and the foramen magnum at the base of the cranium; and after evacuating the brain, and using alternately the crochet and Gardner's tractor (with which I was enabled to exert a most powerful tractile force,) I accomplished the delivery.

The prolonged labor had the effect of inducing so thorough a condition of uterine debility, that it was with the utmost difficulty I could induce an efficient and permanent contraction; and the hæmorrhage after the delivery of the placenta was so alarming that I was obliged to introduce my hand, for the double purpose of removing a quantity of coagula, and stimulating the organ to put on proper action.

The woman made a rapid and satisfactory convalescence.

No. 143 East 13th Street, N. Y.

REVIEWS AND BIBLIOGRAPHY.

Outlines from a course of Lectures on the Principles and Practice of Surgery, delivered by E. Geddings, M. D., Professor of Surgery in the Medical College of South Carolina. Prepared by J. S. WARING, M. D., and SAMUEL LOGAN, M.D., from notes taken during the course. S. G. Courtenay & Co., Charleston: pp. 560.

Reports of lectures are to us always attractive, when cleverly done, from the fact that there is preserved in them a certain freshness, and a peculiar personal influence from the lecturer, which evaporates in the slow distillation of his thoughts in the preparation of a methodical treatise. As text-books for students they are in some particulars more objectionable, but to the practitioner of several years they recall not

only his student days, but many topics of scientific discussion far too apt to be permitted to escape from the mind.

Opportunity has not been afforded us for a minute and thorough reading of all of these lectures of Prof. Geddings, but those to which we have given attention are interesting and suggestive. On many points we should not agree with him, and a minute review of the work would call for many discussions of his doctrines.

But in spite of this we heartily commend the book, and it is perhaps none the worse that it is not one of those which tempts indolent men to follow it implicitly, but rather tends to awaken thoughts in their minds of more real use than blind obedience to any dogmas. The subjects are as varied as those usually embraced in a course of lectures on surgery, and each chapter must of course be short. The editors have themselves contributed some essays on subjects not touched in the lectures, making the volume more complete. It is to be regretted that they did not use greater care in the proof-readings, for there are a great many typographical errors, which a little attention would have avoided.

P.

Contributions to Operative Surgery and Surgical Pathology. By J. M. CARNOCHAN, Professor of Surgery in the New York Medical College, Surgeon-in-Chief to the State Emigrants' Hospital, &c. With illustrations drawn from nature. Philadelphia: Lindsay & Blakiston.

Number one of this series of surgical publications has been placed on our table. It contains the case of amputation of the entire lower jaw, to which we have heretofore had repeated occasion to allude, together with some remarks on that operation; and cases of Elephantiasis Arabum, successfully treated by ligature of the femoral artery. Of this latter disease three cases are now reported by the author, in the last of which both femoral arteries were tied, and the prospects of the patient are very good for entire recovery.

It is clearly unnecessary for us to analyze the cases reported in this *livraison*, familiar as they are to many of our readers. To those who appreciate the luxury of handsome type and good paper we would say, that this publication, by its quarto size and large print, leaves nothing to be desired in these respects.

Two lithographic illustrations accompany this number. That of the amputation of the lower jaw gives a good idea of the various portions of the bone after maceration. The likeness of the patient is quite accurate, though the line of the incisions was not so marked in his face,

when we used to see him, as it is drawn by the artist. One would readily have passed him without suspicion that he had lost his inferior maxillary bone. The patient afflicted with elephantiasis we did not see, and cannot therefore judge of the accuracy of the delineation. It is, however, sufficiently spirited.

The whole series is to consist of ten similar numbers, and being familiar with most of the cases to be described, as well as with the doctrines of the author, we do not hesitate to say that they will constitute a valuable addition to the library of every one who practises surgery.

P.

SELECTIONS.

On the Comparative Use of Ergot and the Forceps, in Labor. By B. FORDYCE BARKER, M.D.

I understand the duty assigned me by the Society to be, to contrast the indications for the use of one of these efficient agents in parturition, in preference to the other. I shall endeavor to define the powers of each, to point out the conditions where each may be made available in assisting labor, and also the indications which forbid a resort to either of these resources. I shall aim to bring the subject up to the present state of science, as derived from the highest obstetrical authorities, based on the most extensive clinical observation, and the soundest and most philosophical reasoning. But in order that I may not occupy too much of the time of the Society, I shall content myself with a condensed statement of the present obstetrical doctrines and practice. To discuss the subject in full, to give the various arguments for and against each principle enunciated, to do full justice by quotations and references, to each contributor to obstetrical science, would extend this paper beyond those reasonable limits, proper for a report to be read before a Society at its annual session.

I shall first speak of the action of ergot, and its use and abuse in labor. There is a peculiar fitness in the effort on the part of this Society, to accurately determine the indications and contra-indications for the use of this article in parturition, as to a former resident of the city where the annual meetings of the Society are always held, who was also for four years its President, belongs the merit of having first called the attention of the Profession to its oxytotic properties. It is now half a century since the letter of Dr. John Stearns,* to Dr.

**Account of the pulvis parturiens: a remedy for quickening child-birth.* In a letter from Dr. John Stearns, of Saratoga County, to Dr. S. Ackerly, dated Waterford, January 25, 1807, is the following narration:

"In compliance with your request I herewith transmit you a sample of the *pulvis parturiens* which I have been in the habit of using for several years, with

Ackerly, was published, and the use of ergot in obstetric practice dates from this period. In the Transactions of the Society there have been published, also, two excellent papers on this article, which have been of great value to the profession. In vol. 3, Dr. S. Chevasse has a paper "on the Injurious effects of Ergot," and in vol. 5 was first published the classical "Observations on Ergot," by Dr. J. B. Beck.

The influence of ergot in inducing uterine contractions in the parturient woman is at the present day well known to the profession. These contractions differ from the normal efforts of the uterus to expel its contents, in that they are continuous, not recurring at intervals. Its effects are manifest in a very short time after its administration, the periods varying in different patients from five to twenty-five minutes. In most cases there is a marked diminution in the frequency of the maternal pulse, following its administration, but the pulse becomes quicker, and remains so, after its immediate action has ceased. In a majority of cases, also, a marked decrease in the pulsation of the foetal heart follows its exhibition. This is succeeded after some time by an irregularity in its beats, which irregularity continues, more or less, until the sounds intermit, and at length, after a variable period, become inaudible. Dr. Hardy states, that in those cases where the number of pulsations have been steadily reduced below 110, and at the same time with intermissions, the child will rarely if ever be saved, although its delivery should be effected with the greatest possible speed.

the most complete success. It expedites lingering parturition, and saves to the accoucheur a considerable portion of time, without producing any bad effects on the patient. The cases in which I have generally found this powder to be useful, are when the pains are lingering, have wholly subsided, or are in any way incompetent to exclude the fœtus. Previous to its exhibition it is of the utmost consequence to ascertain the presentation, and whether any preternatural obstruction prevents the delivery, as the violent and almost incessant action which it induces in the uterus precludes the possibility of *turning*. The pains induced by it are peculiarly *forcing*, though not accompanied with that distress and agony of which the patients frequently complain, when the action is much less. My method of administering it is either in decoction or powder. Boil half a drachm of the powder in a half a pint of water, and give one-third every twenty minutes till the pains commence. In powder I give from five to ten grains; some patients require larger doses, though I have generally found these sufficient.

"If the dose is large it will produce nausea and vomiting. In most cases you will be surprised with the suddenness of its operation; it is, therefore, necessary to be completely ready before you give the medicine, as the urgency of the pains will allow you but a short time afterwards. Since I have adopted the use of this powder I have seldom found a case that detained me more than three hours. Other physicians who have administered it concur with me in the success of its operation.

"The *modus operandi* I feel incompetent to explain. At the same time that it augments the action of the uterus, it appears to relax the rigidity of the contracted muscular fibres. May it not produce the beneficial effects of bleeding without inducing that extreme debility which is always consequent upon copious depletion? This appears to be corroborated by its nauseating effects on the stomach, and the known sympathy between this viscus and the uterus.

"It is a vegetable, and appears to be a spurious growth of rye. On examining a granary where rye is stored, you will be able to procure a sufficient quantity from among that grain. Rye which grows in low, wet ground, yields it in greatest abundance. I have no objections to your giving this any publicity you may think proper."

Now, there are some cases where there is a deficiency of contractile energy on the part of the uterus, and the ergot is a precious resource; but no occasions in obstetric practice require a nicer discrimination or a more accurate diagnosis. We are frequently called upon to decide between the use of agents calculated to excite contraction, and those which have a directly opposite effect, and to determine whether it be better to rouse the energies of the uterus or to resort to manual or instrumental assistance. The safety or destruction of our patient may depend upon our decision. As a means of assisting labor, ergot is admissible only in cases of inertia of the uterus. The differential diagnosis between *inefficient* action of the uterus, and *impotent* action, is therefore of great importance, and sometimes it becomes a question of great difficulty. But there are certain requisites for the admissibility of the drug, which may, at the present day, be regarded as having been established by the experience of the profession.

1st. There must be no mechanical obstacle to delivery. This will exclude all cases where there is disproportion between the size of the fœtal head and the pelvic cavity, all cases where there is even the slightest pelvic distortion, all cases of mal-presentation, and all cases where the obstacle to delivery results from rigidity of the soft parts.

2d. The first stage of labor, viz., dilation of the cervix, must be fully completed. The action of this drug is specially directed to the body of the uterus, and not to the cervix. The physiological functions of the cervix are entirely distinct from those of the body of the uterus, not only when the organ is in the non-gravid state, but also during gestation and at the time of parturition. The phenomena of the first stage of labor pertain to the cervix, those of the second to the body of the uterus. Now, however desirable it may be to overcome delay in the first stage of labor, the attempt to effect this by premature development of the phenomena belonging to the second stage, is attended with great danger, as will be subsequently shown. The only exception to the law, that the first stage of labor must be fully completed, is found in some cases of partial placental presentation, which will hereafter be examined.

3d. The second stage of labor must be so far advanced that the labor can be terminated by efficient uterine action within one hour. The reasons for this law are based on considerations relative to both mother and child. As regards the mother, it requires no elaborate argument to prove that the violent, persistent, and continuous compression of the soft parts beyond this period of time, must be attended with great hazard. Professor Meigs, in the following quotation, has, with his usual felicity of expression, thus made this point: "A labor is effected by the contractions of the muscular fibres of the womb, aided by that of the abdominal muscles. If all the powers employed in a labor could be accumulated in a single pain, lasting as long as all the natural pains do, no woman, probably, could escape with life from so great an agony, except that small number who are met with, and whose organs, happily for them, make no resistance, but open spontaneously, like a door, to let the fœtus out. By a beneficent law of the economy, the pains of

of labor are short, not lasting more than thirty or forty seconds in general, and returning once in three or six minutes." The exhibition of ergot is safe only in those cases where the presentation is natural, the pelvis is well formed, the os uteri well dilated, the vagina and vulva lax and moist, and in short *everything is prepared for delivery, nothing being wanting but efficient action of the uterus*. The dangers resulting from its use are, first, *rupture of the uterus*. In the most excellent monograph on this subject by Dr. J. D. Trask, of White Plains, the imprudent use of ergot is mentioned as one of the prominent causes of this accident, and quite a number of cases are referred to in illustration of the fact. Dr. Trask very justly remarks, "the medical journals, for obvious reasons, contain but few cases of rupture from the imprudent administration of ergot. There can be no doubt that the injudicious exhibition of this drug has been the source of infinite mischief. It is difficult to obtain data upon this subject, for few in whose practice such cases occur would be disposed to report them, and those met with in consultation practice are kept secret from motives of delicacy." The number, however, of such cases that have been reported are sufficient to show that this is by no means an unfrequent cause of this fearful accident. Laceration of the os uteri is another result of the injudicious use of this article. Rupture of the perineum is also a frequent consequence. Dr. Barnes mentions, also, prolapsus and procidentia of the uterus and bladder, as resulting directly from the violent dislocation occasioned by ergot contractions. Other injurious effects upon the mother's system, besides these direct lesions, have been ascribed to the ergot.

Dr. Hardy says, that in several cases where the circulation of the patient had undergone depression from the action of ergot, the effect continued for several days, notwithstanding that, in some instances, inflammation of the uterus followed the delivery, and the uterine tumor not unfrequently remained much larger than natural, even when there was no inflammation. He quotes also, the eminent authority of Dr. Johnson, to the fact that "the volume of the uterus is often found much greater than after ordinary labor, imparting to the hand almost the feel of a uterus, before the expulsion of the placenta."

The fatal effects of ergot on the child, when the delivery is not completed within a limited period of time, are now established beyond all controversy. Dr. Hardy found that in forty-eight cases where the ergot had been given, thirty-four children were still-born—nearly three-fourths. The concurrent testimony of those of the profession, who have not only had experience in the use of this article, but have honestly watched the results on the child, confirm the evidence as to the great danger in its use, although the proportionate mortality is not usually so great as that mentioned by Dr. Hardy. I may mention, as an interesting historical incident, that I have been informed, from an authentic source, as I suppose, that Dr. Stearns, to whom the profession is indebted for the introduction of an article that obstetricians at the present day would find difficult to dispense with, suffered so much in his practice from the mortality among children, charged to the use of this article,

that he left Albany and removed to New York on this account.* It is probable enough, that the enthusiasm of a discovery may have led to some errors in practice, before an accumulated experience had established the laws which should govern its administration, and it is also probable that rival practitioners did not hesitate to take advantage of any such impression. But as showing that Dr. Stearns fully understood the action of the article, and to a great degree appreciated the contra-indications for its use, I again refer to the letter of Dr. Stearns to Dr. Ackerly. (See note, page 39.)

A variety of opinions have been entertained, as to the action of the ergot on the child. Some have believed that it possesses some special noxious property, which is absorbed and transmitted to the child. Others think it acts perniciously, owing to the uninterrupted pressure of the uterus upon the brain of the child, or that the long-continued compression of the trunk, by the uterus, produces a fatal cerebral congestion; but at the present day the best received theory is, that the violent and continued ergotic contractions arrest the utero-placental circulation, and the death of the fœtus results from the want of proper oxidation and decarbonization of its blood.

I have thus far spoken of the value of ergot as a means of assisting delivery, and it will be readily seen that I would restrict its use to a *very limited* class of cases. I will now examine its value for other purposes, connected with parturition.

1st. *As a means of preventing hæmorrhage.* We are safe from the dangers of post partum hæmorrhage only in those cases where the permanent contraction of the uterus is secured. Ergot has often and naturally been relied upon to obtain this result, and of its value for this purpose there can be no question, except in those cases where the labor has been so prolonged that not only the nerve power of the uterus, but also that of the general system, has been exhausted. In these cases our greatest security is found in the liberal use of opium, or of some of its preparations. The time when the ergot should be administered as a means of preventing hæmorrhage, is a matter of a good deal of importance. Some have advised that it should be given when the head of the child is on the perineum, and about to be expelled. Others delay its use until after the head has cleared the os externum, but before the shoulders have passed. McClintock and Hardy prefer to give it as soon as the insertion of the cord into the placenta can be felt. Dr. Hardy remarks, that, "by giving the ergot before the child has been expelled, some time may be gained; but should the placenta be morbidly

* From Dr. Willard's Semi-Centennial Address before the Medical Society of the County of Albany, published in the Transactions of 1857, we copy the following:

"In a somewhat extensive and successful practice, Dr. Stearns became unfortunate in losing a series of cases of puerperal fever. It was not then, as now, understood that this disease is contagious, and may be communicated from one lying-in patient to another by the hand or clothing of the accoucheur. The mystery of its appearance in his practice only, and the fatality of its termination, keenly oppressed his truly sensitive mind, and led him at length to abandon his practice in this city."—*Com. of Pub.*

adhering to the uterus, the difficulty of introducing the hand for its removal will be greatly increased. By adopting the third plan this source of apprehension is avoided." But morbid adhesion of the placenta is not a very frequent occurrence, and in those cases there is reason to apprehend danger on account of previous hæmorrhages. I should not hesitate to use the ergot at an earlier period.

2d. *As a means of arresting hæmorrhage.* In partial presentation of the placenta, the ergot is often strikingly beneficial in controlling the hæmorrhage. It is in these presentations only that I would ever use ergot in the first stage of labor. The tampon is to be relied upon, until the os is considerably dilated; but this is no longer efficient when the os is tolerably open, thin and soft, as the yielding membranes will not afford a counter-pressure. The membranes are now to be ruptured to allow the head to descend so as to compress the placental vessels; and now, to secure the permanent tonic contractions of the uterus, and thus prevent further loss, the ergot becomes invaluable.

The ergot has been strongly recommended by some as a means of arresting the hæmorrhage which occurs after the delivery of the child, but before the expulsion of the placenta; and if we could always be sure that the retention was the result of uterine inertia alone, the propriety of its use would never be doubtful. The indication in these cases is, the prompt removal of the placenta. Now, if the placenta is morbidly adherent, the use of the ergot would render its detachment much more difficult. If, on a careful examination, this condition is ascertained to be absent, the ergot may be used with advantage, not so much for the purpose of facilitating the expulsion of the placenta, (for this must be effected at once by a manual operation,) as for the sake of securing the permanent contraction of the uterus after its delivery.

As regards the hæmorrhage which occurs after the delivery of the placenta, the distinction made by various authors (first clearly pointed out, as I believe, by Dr. Beatty, of Dublin,) as to the two opposite conditions which favor this formidable accident, I regard as very important. "The first is the full, plethoric habit, where the heart is in strong and rapid action, and all the vessels are gorged with blood, as is indicated by the flushed skin, headache, thirst, and bounding pulse. The second, is the weak, delicate, lax fibred state, characterized by pale countenance, spare limbs, slow and weak labor pains, and feeble, though it may be rapid pulse." In the first class, hæmorrhage within certain limits may be beneficial, and it is in these cases that the use of ergot proves especially valuable, from its known power of lowering the circulation, as well as of securing the permanent contractions of the uterus. We have no fear of its depressing influence, and we can rely with a good deal of certainty upon its power, in these cases, of securing the latter object.

But the propriety of its use in hæmorrhage occurring in the second, is often doubtful. In these cases, it does nothing to excite the contractions of the exhausted uterus, its depressing influence producing just the opposite result. I must especially protest against the large doses which are in these cases sometimes administered, with the vain

hope of stimulating the exhausted organ, the only effect of which is, still more to debilitate it. I can positively aver, that in more than one instance I have seen death result from its injudicious administration under these circumstances. Without stopping to describe the various methods to arrest the hæmorrhage, which should be resorted to in these cases, I will simply remark, that the ergot should never be administered except in conjunction with opium, or until the exhausted powers of the nervous system have been restored by the use of opium. "In these cases," says Dr. Lever, "where there is great exhaustion, alarming syncope, great irritability, severe vomiting, and pain, evident and undeniable indications of great depression of the sanguiferous and nervous systems, or, to use the graphic language of Dr. W. Griffin, 'when the countenance is sunk, the eye hollow and glassy, the lips blanched, the skin cold, and the whole person corpse-like; when the pulse is all gone at the wrist, when the beat of the heart is scarcely perceptible, and stimulants, even brandy, are vomited or useless, opium will act like magic, and save the patient from an untimely grave.' But to do good, it must be exhibited in full doses of one or two drachms of the tincture, or three or four grains, repeating two grains every half hour or hour until the pulse becomes distinct, the breathing calm, and the jactitation allayed; whatever may be the '*ratio medendi*,' whether the congestion produced in the brain be what is necessary to maintain the proper tension of the cerebral vessels, whether it restore the loss of nervous power in the brain itself, is still a point *sub judice*; but no man of much obstetric experience, will deny its value under the circumstances thus detailed. As pertinent to the foregoing remarks, I may be excused for making the following quotation from Prof. Murphy: 'The paradox has been proposed, how can opium cause the uterus to contract in hæmorrhage and to relax in other cases? for instance, when given for this purpose in arm presentations. The same medicine cannot produce opposite effects on the same structure. In this query, the condition of the nervous system, a most essential element, is totally overlooked, and the influence of opium, where nervous irritability is almost exhausted, is compared with its effects when the same power is excited to the greatest degree. It is assumed that the operation of opium must be the same when the uterus has lost all power to contract, and when it has contracted spontaneously. The question, therefore, might be easily answered, by stating that opium is both a stimulant and sedative, and that one effect or the other is produced, according to the relation existing between the nervous energy of the uterus and the dose of the medicine given. If nervous irritability be not impaired, or if it be increased, a very small dose of opium would stimulate—a larger one would exhibit its sedative effects; but, if on the contrary, that irritability is destroyed, and the uterus atonic, the same large dose would only act as a stimulant; nor will the sedative property of the medicine be observed until the nervous energy is restored. In the use of opium, therefore, strict attention should be paid to the degree of hæmorrhage, and its effect upon uterine contractability. When the loss of blood is slight, or at least not sufficient to impair the tone of the

uterus, a large dose of the opium would be dangerous, lest it might act as a sedative, overcome the influence of the nerves, and cause the uterus to relax. When the loss is great, and followed by exhaustion of the uterus, then the very same quantity of the medicine will produce an opposite effect—it will act as a stimulant, and cause contraction of the uterus.”

As a remedy for retention of urine after labor.

This is generally the result of a temporary paralysis of the muscular coats of the bladder, arising from over-distension during labor. For some years I have not had occasion to use the catheter on account of this condition, the ergot in every instance having proved efficient. I usually give the tincture in doses of 20 drops, repeated every half hour, until the bladder is relieved.

The forceps in labor.

It is not my province in the present paper either to give the history of the discovery of these instruments, or to describe their mode of application, but solely to discuss the indications for their use, the dangers attendant upon their use, and thus to contrast their value in labor, as compared with the use of ergot. There is a great difference in the teaching of different obstetric schools, relative to the indications for their use, and the comparative frequency with which they are required. Before discussing the disputed points, let us briefly examine those where all agree:

1st. As to the functions of these instruments, all are agreed, that they are to be used as a means of assisting the uterus to expel its contents, by their tractive powers, and also for the purpose of correcting any defective mechanism in the labor. Traction alone, is far from being the most important aid to be accomplished by them. Flexion, extension, or rotation of the head is in many cases more important and necessary than traction.

It is unnecessary for me here to insist upon the great importance of thoroughly understanding the normal mechanism of labor. I suspect that by far the most frequent mistake made in the use of the forceps, is in employing them simply as tractors, and that to this cause is due most of the unfortunate results which have followed their injudicious use.

2d. All schools teach that the forceps are to be used where the conditions of the labor will permit, in all those cases where the powers of nature have been well ascertained to be insufficient to accomplish the delivery with safety to the mother and child. I will subsequently allude to the differences of opinion as to the conditions which admit the use of the forceps.

3d. All agree that, where the forceps afford the most feasible and safest mode of accomplishing the delivery, they should be used in all those accidents of parturition which require a speedy termination of the labor.

Thus, in severe puerperal hæmorrhage, it is a settled rule of practice, that delivery should be speedily effected by means of the forceps, when

the os is well dilated, the head is in the pelvic cavity, and there is no mechanical obstacle either from pelvic deformity, tumors in the cavity, or abnormality of the foetal head. So in puerperal convulsions, if after a careful investigation, it is evident that the convulsions are excited by the reflex irritation of the peripheral extremities of the nerves of the cervix uteri, or of the soft parts, there is no hesitation as to the propriety of using the forceps at once, if their use is practicable. For example, where the pains are feeble, infrequent, and inefficacious, or even if the contractions are energetic, but the convulsions are frequent and prolonged, with coma in the interval of the paroxysm, the safety of both the mother and child require the immediate application of the forceps.

When rupture of the uterus occurs, the os being dilated or dilatable, and the head remaining in the pelvic cavity, the forceps must be applied at once and speedy delivery effected.

In cases of prolapsus of the cord, accompanying vertex presentations, the safety of the child requires that delivery by the forceps should be accomplished before the pulsations of the cord cease. It would be a waste of time to discuss these points, as there is no difference of opinion in the profession as to the propriety of the rules above enumerated.

Dangers resulting from the use of the forceps.

The only danger to the mother results from the increased liability to hæmorrhage from inertia, on account of the rapid evacuation of the contents of the uterus, and this liability can always be prevented by proper precautions, and the use of the known means to secure immediate and permanent contraction of the organ. Injury to the soft parts of the mother can only result from the abuse of the instruments, and not from the proper use of them. The rules for their use are now as well established as any principles in obstetric practice, and to condemn any resource of art on account of its liability to abuse, is not only illogical, but opposed to the progress of science. This point will be examined more in detail in a subsequent part of this paper.

The dangers to the child are more numerous. The necessary compression by the instrument may inevitably cause contusion, laceration, and even partial separation of the scalp, and possibly cerebral effusion, depression and fracture of the cranium. These dangers are not to be forgotten in any case, when the propriety of using the forceps is under consideration; but they are not to overweigh the question of safety to the mother. Quite recently the attention of the profession has been called to the danger of facial paralysis in the child, resulting from the pressure of the blade on the seventh pair of nerves. This accident is very rare, and in all cases hitherto reported the paralysis fortunately has proved only temporary in its character.

I have thus far spoken only of those points in regard to which all obstetricians are agreed, as to the indications for the use of the forceps, and the dangers attendant upon their use. There remains to be discussed the rules for practice, which have not yet been established, and the great diversity of opinion will be striking, from the following

statement as to the comparative frequency with which these instruments are used by the most celebrated obstetricians in Europe. Ramsbotham used the forceps once in seven hundred and twenty-nine cases, Joseph Clark once in seven hundred and forty-two, Collins once in six hundred and seventeen, Churchill once in five hundred and forty-six, Lever once in five hundred and eighteen, Simpson once in four hundred and seventy-two, Lachapelle once in two hundred and ninety-three, Beatty once in one hundred and thirty-one, Merriman once in ninety-three, Nægele once in thirty-one, Carus once in fourteen, Siebold once in seven, G. Hamilton once in seven. One explanation of the great difference is found in the fact that the great majority of British obstetricians resort to craniotomy in all those cases where the head of the child has not passed completely down into the pelvic cavity, while the continental obstetricians in the same class of cases would use the forceps. Thus Collins used the perforator once in one hundred and forty-one cases, Churchill once in one hundred and forty-nine, while Nægele used the perforator once in one thousand seven hundred and eleven cases, Mad. Lachapelle once in one thousand eight hundred and fifty-four, and Siebold once in two thousand and ninety-three. Professor Simpson has shown, from a comparison of the statistics of the three great lying-in hospitals of Vienna, Paris and Dublin, that the proportion of cases of operative or artificial delivery is very nearly the same: "In the Vienna hospital, under Beer, one out of every fifty-five women was delivered either by the forceps, vectis, craniotomy or version, and during the time of Dr. Arneth's report, one out of every sixty-nine cases. In the Paris hospital, Mad. Boivin reports one out of every sixty-one labors as requiring delivery by operation. Mad. Lachapelle found that in the ten years preceding 1810, one in fifty-seven mothers were delivered artificially, and during the subsequent ten years, one in eighty-two required such a procedure. In the Dublin Hospital, Dr. Collins reports one out of every eighty-six women as having been delivered artificially, and Drs. McClintock and Hardy describe one out of every fifty-two of their cases as having been similarly assisted." The difference of practice, then, consists not so much in difference of opinion as to the propriety of rendering artificial aid, as in the choice of means for this purpose. The laws which must govern the decision as to the choice of means, cannot be drawn from statistical tables, but must be deduced from certain general principles; and following these laws, it may happen that one who has a large consultation practice may find the forceps necessary once in every ten cases of labor that he meets with, while if his consultation cases are excluded, the proportion of forceps cases will not be more than one in a hundred. Or again, an obstetrician may meet with five cases requiring the forceps in every hundred cases of labor, during the first ten years of practice, when a large percentage of his cases are primipara, when in the second ten years he may not find the forceps necessary oftener than once in a hundred cases, as the proportion of primipara cases changes in the same ratio. I think it sufficient merely to state the above proposition to show the fallacy of relying upon statistical tables to establish the principles which should govern practice.

It is, however, evident, from the statistics before given, that there is a wide difference of opinion among our obstetrical authorities as to the class of cases requiring the forceps, one class never using them except when the evidence of positive arrest at the lower strait is already demonstrated, by the time which the labor has occupied, and the urgent symptoms accompanying it. They require from six to eight hours of ineffectual struggle on the part of the uterus, and the development of symptoms indicative of danger either to mother or child. Without stopping to discuss in detail the special teachings of different authorities, I may say that the plain, practical question to be decided in the lying-in room is, which is safer for mother and child, *the use of instruments or further delay?* The dangers resulting from the use of the forceps have already been noticed, and in my judgment are sufficiently strongly enforced in our systematic treatises, while the dangers of delay have received but slight attention, as compared with the importance of the subject.

Prof. Simpson has shown that the maternal mortality attendant upon parturition increases in ratio progressive with the increased duration of the labor. He has made out the following table, showing the proportion of 138 natural deaths in relation to the duration of labor in 15,850 cases of delivery recorded by Dr. Collins:

Duration of labor.	No. of deliveries.	No. of deaths.	Proportion of deaths.
Within one hour.....	3,537	11	1 in 322
From 2 to 3 hours.....	6,000	26	1 in 231
From 4 to 6 hours.....	3,875	29	1 in 134
From 7 to 12 hours.....	1,672	21	1 in 80
From 13 to 24 hours.....	502	19	1 in 26
From 25 to 36 hours.....	134	8	1 in 17
Above 36 hours.....	130	24	1 in 6

So also the infantile mortality attendant upon parturition increases in ratio progressive with the increased duration of the labor, as is shown in the following table of the proportion of still births, in reference to the duration of labor in 15,850 cases of delivery:

Duration of labor.	No. of deliveries.	No. of still born.	Proportion.
Within 2 hours.....	7,050	347	1 in 23
From 3 to 6 hours.....	6,362	346	1 in 18
From 7 to 12 hours.....	1,672	151	1 in 11
From 13 to 24 hours.....	502	88	1 in 6
From 25 to 36 hours.....	134	42	1 in 3
Above 36 hours.....	130	71	1 in 2

It will thus be seen that the dangers of delay, both to mother and child, becomes a question of the gravest importance. Among our systematic authors, Burns has more strongly, and I think more truly pointed out these dangers than any other of our English writers. He says the continued pressure of the head on the soft parts is productive of further diminution of the capacity of the pelvis, for inflammation is

excited, and at the same time the return of the blood by the veins is obstructed, and of serum by the lymphatics. This impairs the power of the soft parts, and renders the inflammation of the low kind, so that even when delivery is accomplished sloughing succeeds, whereby very dreadful or loathsome effects are produced, if these, indeed, be not prevented by the death of the patient, in consequence of a similar low inflammation being communicated to the peritoneum. This swelling of the parts contained within the pelvis may take place, although the head be not impacted, but the head cannot long be impacted without producing that.

Here, then, is one effect of a most formidable and alarming nature, which we apprehend in the case under consideration. But this is not the whole of the evil; for the upper part of the vagina, or the cervix uteri, may be lacerated in consequence of this debilitated state, or any part of the uterus may be ruptured by strong and spasmodic action; or uterine or peritoneal inflammation may be excited previous to delivery, proving fatal in a few hours after labor is terminated; or hæmorrhage may occur, to a fatal degree, from want of energy in the uterus after delivery; or general inition and exhaustion are produced; the pulse becomes frequent, and at last feeble; the mouth parched; the skin hot; the mind confused, and the strength sunk; or the powers of life may be worn out, so that the patient shall die without any decided inflammation or disease referable to a common nosological system. In the Clinical Midwifery of Dr. Robert Lee, who is no advocate for the frequent use of the forceps, and, indeed, who never uses them except when the head is at the lowest strait, occurs the following statement, which seems to me very significant: "In thirty-eight cases of this report the labor continued from forty to seventy hours. In the cases of spontaneous rupture of the uterus and convulsions only was the delivery effected before the labor had lasted upwards of thirty hours. In a very large proportion of the cases the difficulty arose from distortion, or a contracted state of the pelvis. Rupture of the uterus took place in three before perforation; and the inflammation and sloughing of the uterus, vagina and bladder, which proved fatal in eight hours, were chiefly or solely produced by the long-continued violent pressure on the soft parts, by the head of the child before it was opened and extracted. In those who recovered with vesico-vaginal fistula, or contraction of the vagina from cicatrices, the unfortunate occurrence arose from craniotomy being too long delayed." In eighty-seven of Dr. Lee's cases, where craniotomy was performed, local lesions on the part of the mother are noted as having occurred in several instances. Out of the eighty-seven cases, eight, or about one in every ten, suffered from vaginal inflammation and sloughing; four, or nearly one in every twenty, were left with vaginal fistula. In a paper on the subject of "Urethro-vaginal and vesico-vaginal Fistulas," published in the North American Med. Chir. Review for July and November, 1857, by Dr. N. Boseman, of Montgomery, Alabama, he states that in nineteen cases of these fistulas "the shortest duration of labor in any one of these cases was thirty-six hours, and the longest eight days; the average being about

four days. In nine of these cases instruments were employed to aid in the delivery; in six no artificial means were resorted to." He adds: "Judging from the nature of the fistulous openings in the cases where instruments had been used, and where they had not, I am forced to the conclusion that nearly, if not all of them, were the result of sloughing." In further confirmation of the views advanced as to the danger of delay in labor, I add a note from Dr. Sims, who has undoubtedly had a larger experience in the lesions resulting from parturition than any man living:

79 MADISON AVE., Jan. 30, 1858.

My dear Doctor—Out of about one hundred and twenty cases of vesico-vaginal fistula, I have had time to look over the histories of only seventy. Of these forty-one were delivered by instruments, the rest being left to the unaided efforts of nature.

These fistulas are sometimes produced by laceration, but most commonly by a slough which is generally in proportion to the duration and degree of impaction, whether instruments are used or not. Instruments are often blamed for injuries which are produced, not by their use, but by the want of their timely application; in other words, by the prolonged pressure resorted to.

The cases left entirely to the unaided efforts of nature, other things being equal, suffered the greatest loss of structure; those in which instruments were used, sustained, as a rule, less loss in proportion as they were resorted to early or late, thus showing that the mischief was the result of prolonged pressure.

With great regard,
Ever sincerely yours,

J. MARION SIMS.

Prof. FORDYCE BARKER.

In conclusion, I must state my conviction that the more enlarged is the clinical experience, and the more accurate the observation, the more rarely will the ergot be used before delivery, and furthermore, that the fear of delay in labor will be greater than the apprehension from the use of forceps. McClintock and Hendy gave the ergot in the second stage on account of inertia in nineteen cases, after which it was necessary to deliver with one or both blades of the forceps. In ten of these cases the child was lost. The death of the child could not have resulted from the use of the instruments, and must have been due either to the ergot or the protracted labor.

Toxic and Medicinal Properties of Nitrate of Oxyde of Glycyl. By
A. G. FIELD.

In the evening of February 3, 1858, I was conversing with a homœopathic practitioner, when he mentioned a medicine which possessed peculiar and extraordinary qualities, some of which he described as having affected himself, though he had taken it in very minute quan-

ties. I laughed at his credulity, and offered to take as much as he pleased, upon which he let two drops of what he called the first dilution of glonoine fall on my tongue. After swallowing this small quantity of fluid—I was assured the quantity did not exceed two drops—I asked what effects I must expect, but was told to wait and observe for myself. I then purposely conversed on other subjects. In about three minutes I experienced a sensation of fulness in both sides of the neck; to this succeeded nausea, and I said, "I shall be sick." The next sensation of which I was conscious, was as if some of the fluid was being poured down my throat, and then succeeded a few moments of uncertainty as to where I was, during which there was a loud rushing noise in my ears, like steam passing out of a tea-kettle, and a feeling of constriction around the lower part of my neck, as if my coat were buttoned too tightly; my forehead was wet with perspiration, and I yawned frequently. My intellects returned, however, almost immediately, and I remember saying, "This has nothing to do with homœopathy, but it has to do with a very powerful poison; there are more things in heaven and earth than are dreamed of in the philosophy of some of us." I also reproached my friend for not having tested the anæsthetic power of the medicine, by inflicting a slight wound on me. I need scarcely say I am thus minute in my description of what occurred, that an accurate idea may be conveyed of the actual effect produced on me, as well as to justify the uses to which I have since put the medicine. When these sensations had passed off, which they did in a minute or so, they were succeeded by a slight headache, and dull heavy pain in the stomach, with a decided feeling of sickness, though without any apprehension that it would amount to vomiting. I lay on a sofa, feeling rather languid, but talking cheerfully, conscious at the same time that I could very well exert myself both mentally and physically, if I liked, but that it was more pleasant to be idle. This condition lasted about half an hour, at the end of which I was quite well, and walked home, a distance of half a mile, with perfect comfort. I slept soundly from one o'clock till six, when I was called up, having a slight amount of general headache, but not such as I should have regarded but for the recollection of last night's adventure.

The physician to whom I am indebted for this over-dose, told me, that when his first impression that I was shamming had passed off, my condition caused him the greatest alarm, for he really thought he had killed me. I learn from him that my head fell back, my jaw dropped, I was perfectly white, breathing stertorous, and no pulse at the wrist for the space of about two minutes. He immediately rushed to a closet, and procured some stimulant, which he poured down my throat. I had never been in better health and spirits than on the day of this occurrence, and had taken nothing for hours but a little cold tea.

This same first dilution of glonoine consists of one drop of a peculiar chemical compound, dissolved in ninety-nine drops of rectified spirit; and glonoine itself I learn to be a nitrate of oxide of glycy, prepared by adding nitric and sulphuric acids to glycerine, the temperature of the fluids being kept down by a freezing mixture.

My own personal experience of the very marked and peculiar effects produced by this drug made me anxious to test its qualities still further. As a direct sedative to the nervous system, without possessing any stimulating or permanently depressing qualities, without affecting secretion, together with its power of subduing muscular action, it appeared to promise to become an invaluable agent in the treatment of a large class of nervous and spasmodic diseases. By a strange perversion of all reason, as it appears to me, my friend, who is an enthusiastic disciple of Hahnemann, began to rejoice, when all appearance of danger had passed, that he had discovered what he considered a splendid remedy for apoplexy, on the principle of *similia similibus curantur*. I leave him to the enjoyment of his opinions, feeling only grateful that he did not give me a second dose to cure me on a like principle, while I consider the best mode of applying the drug in a precisely opposite direction. With this object I procured some of the first dilution of glonoine from a homœopathic chemist, and proceeded to institute a series of experiments before applying it to the treatment of disease.

Anxious to inform myself on the effects of a smaller dose, I got a medical friend to join me. We each touched our tongue with the cork moistened with glonoine solution, and recorded the sensations produced by it. They were nearly as possible identical—a sense of constriction of the neck, slight nausea, with fullness, and some pain in the head, as if the brain were expanding. But I think my friend must have experienced more decided effects than I did, for he declared he would never take any more. The sensations lasted about five minutes, and then passed off without leaving any unpleasant effects.

Animals, as far as my experiments have extended, appear to be almost unaffected by this drug, which acts so powerfully on the human organization.

I have repeatedly given it to cats, rabbits, and other animals, in doses varying from two to thirty drops, without producing any immediate effect. One rabbit had diarrhœa an hour after, and the cats appeared cold and lazy all the next day. Some smaller animals, such as mice and pigeons, died after having taken the glonoine some hours, but they appeared to have suffered from alcoholic poisoning, rather than from any symptoms at all resembling those produced by the glonoine on the human subject.

Disappointed in my endeavors to gain any information from experiments on animals, I still thought I had seen and felt enough of the physiological action of the medicine to justify my cautiously employing it in the treatment of disease.

"Case 1.—Mrs. L., aged 68, had for some days been under treatment on account of a very painful nervous affection, which she designates spasms. This recurred regularly every three hours, and is described by herself and attendants as most distressing, and my own observation of one or two seizures fully bears out their statements. Each attack commenced suddenly with intense pain in the epigastrium, extending up to the top of the chest, and then down the inner

side of the left arm; it lasted about half an hour, and then subsided, leaving her exhausted, but otherwise well in the intervals. They recurred during the night with equal regularity. She was at the same time the subject of uterine derangement. Fetid ammonia, assafoetida, chloroform, valerian, hyoscyamus, camphor, and prussic acid, with counter-irritation, having failed to give her relief, I had recourse to morphia every two hours, which relieved her only after several doses had been taken, and partial narcotism had been produced. She would then enjoy a few hours' peace; but the attacks always returned—when the influence of the morphia had passed off.

"Feb. 5th.—She had slept well all night from the morphia which had been taken in the previous twenty-four hours, and was awake in the morning of this day by one of her painful attacks; but it yielded in three minutes to a quarter of a drop of the solution of glonoine in a dessert spoonful of water. After this she had four more attacks before noon. For three she took the same medicine, and was quickly relieved; but having exhausted her supply when the fourth occurred, she suffered as much as on former occasions.

"My daily notes of this case are nearly a repetition of what I have just stated, till the evening of the 10th, when she appears to have taken an over-dose, which produced effects very similar to those from which I suffered on the 3d. This gave rise to so much alarm, that she refused to take any more. I therefore again had recourse to morphia; but she suffered so severely the next day and night, that she begged to be supplied with the glonoine again, and no sooner had she taken it than relief was obtained. The dose has been continued every four hours, with the happiest results. Her attacks now are reduced to two or three in the twenty-four hours, and always readily yield to the quarter of a drop of solution of glonoine. The only other treatment she has required has been a few ten-drop doses of the tincture of *Cana-bis indicus*, to relieve uterine hæmorrhage.

"Case 2.—Mrs. W. had suffered severe pain from a decayed tooth for several hours. The pain was so great, that she would gladly have had it extracted; but her dentist was anxious to preserve it. In the evening she begged me to give her something, for she said, 'It cannot be made worse.' I placed about half a drop of the solution of glonoine (1 per cent.) on her tongue. Soon after she experienced a pulsation in the neck, fulness in the head, throbbing in the temples, and slight nausea. The toothache subsided, and she became partially insensible, disliking very much to be roused. When fully sensible, she had headache, but the toothache was gone. Mr. W. remarking, 'Certainly that medicine allays pain wonderfully.' She slept unusually well that night, and experienced no ill effects in the morning.

"Case 3.—Elizabeth M., a stout healthy young woman, had severe toothache. I was applying a very small piece of lint dipped in glonoine solution, (1 per cent.,) when it accidentally fell into her mouth and was swallowed. In about five minutes, after feeling giddy and sick with headache, she became insensible. Her countenance, naturally florid, was unaltered, breathing tranquil, pulse full, and rather quick-

ened. Knowing, as I did, that she had taken but a small quantity of the drug, I kept my fingers on her pulse, and allowed myself time carefully to observe her condition before applying a restorative. I tested her sensibility to pain, and called loudly to her, but without producing any impression. Directly I detected a slight failure in the pulse, in about three minutes after insensibility commenced, she had some stimulant poured down her throat, when she quickly recovered. Some headache was complained of, but the toothache was cured. The next morning she was quite well.

"Case 4.—Mrs. R., aged 45, pale, anæmic, with feeble circulation, has for the last month suffered from headache, daily increasing in severity. When I first saw her, February 15, she had had leeches applied to the temples, and had taken drastic purgatives, since which the pain had been much worse, and she could not sleep. I gave her a quarter drop of glonoine solution in colored water every four hours. On seeing her the next day, she expressed the greatest gratitude for the relief the medicine had afforded her, and she said her head was much better after taking the first dose, and she slept four hours. The glonoine was of course given only as a palliative in this case, while iron and generous diet were relied on as a means of effecting a cure.

"I have not yet met with one well-defined case of neuralgic or spasmodic disease in which this medicine failed to afford relief. No vague, over-sanguine expectations are entertained of its power to cure disease where spasm or pain are but symptoms, excepting only in those cases where these consequences themselves become the cause of death, their cause being of a transient nature, and liable to subside if the patient's life can be maintained for a certain time, such as temporary irritation of a nervous centre, or inflammation of such a part, which might terminate in resolution or be subdued by remedies, if existence were prolonged sufficiently for their action; and also in cases where we may suppose symptoms such as spasm may react on their exciting cause, preventing the necessary tranquillity for recovery, the offspring, as it were, maintaining its parent. With such a remedy, may we not look forward hopefully to the treatment of tetanus, hydrophobia, and other similar diseases?"—*Med. Times and Gaz.*, March 20th.

A Clinical Lecture on Pus and Suppuration. Delivered at the Hôpital de la Charité. By M. VELPEAU.

Before you begin to observe diseases, every variety of which is to be met with in these wards, it would be well to study, in a clinical point of view, certain subjects which it is important you should understand, and the more especially so, as they are of such frequent occurrence. But of all these there is none that occupies so important a place, or occurs so frequently, as pus. If the immediate consequence of every solution of continuity, whether surgical or accidental, be effusion of blood, so also is the formation of pus one of its very constant results,

Blood flows out wherever you give it issue; nor is there any part where pus, which may be styled a product of surgery, may not form. In practice it is met with at every step, and its presence is always an index to disease; it is consequently a sign of great value, which may itself, whether maintaining its normal characters or showing evidences of decomposition, become in turn the source of new and formidable symptoms.

Pus has not always the same characters. There is a type—the phlegmonous, *laudable* pus of authors, described as cream-like, white, thick, and unctuous, without flakes or clots, is homogenous and consistent, such as is found on opening abscesses formed in the cellular tissue. But pus varies, besides, according to the nature of the organs in which it is formed. Thus pus in the liver is red or brownish; at the anus it often has the color and odor of fecal matters; and is modified throughout the whole extent of the mucous coverings by the proximity of these membranes to the organs which they protect, so that the practitioner with an exercised organ of smell may become able to distinguish infallibly whether pus is from the mouth, throat, or larynx, from the air passages, or from this or that portion of the alimentary canal. Pus varies also according to the constitution of the sick. In persons that are sanguine, robust, and abundantly endowed with cellular tissue, it is thick, fat, and creamy; while in the meagre, sickly, and exhausted, its condition is just the reverse—it is serous, clear, and grumous. The pus of bones is clear, yellow, and serous. Cancerous ulcerations and gangrene give rise to a fluid which is a dirty serous discharge mingled with decomposed blood and detritus, rather than a true suppuration.

Pus forms in two sorts of foci that are quite distinct; at one time the focus is the surface of a wound, when the pus is in contact with the external air; at another time it is in the substance of a limb, among the tissues in the natural cavities, the articular capsules, the synovial bursæ mucosæ, the serous cavities, the peritoneum, the pleura, etc., when contact with the atmospheric air is precluded. We shall consider it in each of these two cases, for its history in the one instance is different from what it is in the other. Effused into the interior of the body, and having no communication with the external air—a circumstance of much importance—it is no longer either more or less than a foreign body, soft and liquid, which preserves, unchanged, its primitive characters, and against the infiltrations of which into the neighboring parts nature takes her precautions; hence, some days after its formation, the containing cavity is found to be lined throughout, even to its minutest sinuosities, with a membrane soft to the touch, tomentous, and velvety—the pyogenic membrane, which isolates the purulent focus from the rest of the system. In the shut cavities it is the serous membrane that fulfils the office of the pyogenic, especially in the joints. As the serous membrane covers tissues at different degrees of density, the pus is irregularly and imperfectly confined; hence, sometimes it bursts through its barriers into the neighboring parts; or, as happens in other cases, the parts become greatly distended, giving rise to inflammatory and other formidable phenomena.

Where pus forms on the surface of a wound, being subject to the laws of putrid fermentation from the influence of atmospheric air, it undergoes serious modifications. Not only do the elements of which it is composed become dis-associated, but they form new combinations in virtue of their chemical affinities; hence, new compounds, the nature of which you should understand, are formed—ammonia, sulphurets, sulf-hydrates, various compounds of hydrogen, carbon, &c., presenting so many causes of infection, against the influence of which the system has to re-act in order to defend itself, and of which the surgeon, knowing their formidable consequences, should never lose sight.

Suppuration has a constant tendency to perpetuate and extend itself; the moment pus occupies any part of the body there is reason constantly to fear the implication of the whole system, or at least the formation of new foci in other regions. It is not unlike an enemy besieging some stronghold. You watch it, supposing that there it has concentrated its whole force; but all at once the enemy decamps, only to muster in greater force where least expected. It has its own peculiar strategy, ever ready to take advantage of the least oversight and of every inequality of ground; and when seeming the least offensive becomes suddenly the most destructive. How often, in fact, have we not seen, for example, an abscess following, to all appearance, its march to a regular termination—the suppuration, of good quality, becoming every day less and less, so as to leave no doubt of a speedy cure? How often have we not seen, under such circumstances, repeated suppurations occur in parts distant from the primitive focus in the viscera themselves, and become the definite cause of death, even when you would, some days before, have affirmed that a perfect cure was at hand?

It was in former times supposed that when in the course of a disease there was either a sudden or slow dispersion of pus, and pus was seen collecting in another part, there must be a transportation of pus *proprio motu*; hence the belief in its metastasis. But since these phenomena have been better studied, a more accurate explanation has been given. The secondary suppuration seen in such cases is not the result of any circulatory movement; for neither is the pus nor its elements carried about from one part to another. The pus that has just shown itself is the result altogether of a special and peculiar diathesis, manifested by every individual in whom suppuration occurs, so that he becomes liable, from the very existence of one abscess, to other abscesses in endless succession, and that either during the existence of the first, or long after, in its vicinity or at a distance.

Pus, therefore, engenders pus—a proposition neither new nor questionable. An individual in whom a purulent secretion occurs, let the part involved be what it may, becomes so exposed to the influence of the pus that he may thence become as thoroughly infected as he who, from a single variolous pustule, becomes completely covered with confluent small-pox. Moreover, the notion of pus being transported from one part to another by metastatic action, is refuted by mere attention to the facts. Have we not seen a score of persons die from purulent

infection, whose bodies were in various parts inundated by a mass of pus far more considerable than the primary collection of pus that gave them origin?

Sometimes it happens that pus disappears entirely from a part where it lodged, yet causing no new accident, terminating thus by resolution, when the system is supposed to get rid of it through the instrumentality of the excretories. But in this case, as in the former, the fact is not easily explained. The pus was supposed to be removed either in its state of integrity, or reduced by the veins and absorbents to its primary elements, and thrown thus into the circulating current. But before we can determine the intimate nature of the process, it is plain we must be acquainted likewise with its intimate composition—a subject on which it must be owned we possess nothing very definite. Pus is said to have a special globule, and some have gone so far as to define its characters and properties; but the very existence of a pus-globule, which would thus constitute the principal element of pus, is not even admitted by all who devote themselves to micrographic studies. The danger from absorption is much diminished when the pus absorbed, unlike that produced by the surface of a wound, has not been exposed to contact with atmospheric air. In fact, although it is an anomalous and morbid body, yet its elements have still preserved their primary properties; they are still animal particles, that may be returned without inconvenience into the system from which they had been separated. But in other cases, as we have said, they are new bodies that go to poison the system. In fact, soon after absorption, particular symptoms declare themselves, and render manifest their pernicious action on the organism, such as tremblings and those remarkable rigors of which we all know the import—the precursory signs of an adynamic fever that never fails to carry off the patient.

Independently of its general properties, we must also look at pus in a practical and surgical light. When the wound from which it issues is large and inclined, it finds a ready exit; the secreting surface gradually lessens, and there is good reason to look for a cure; but should the wound, on the contrary, be deep and anfractuons, and its situation such as to be unfavorable to the discharge of the secretions, the pus then lodges and corrupts at the bottom of the wound, and it is then you have reason to apprehend the accession of those symptoms of purulent infection we have mentioned. The anatomical disposition of the parts directs, as it were, the pus in its course, and the cellular tissue opens up the way. When lodged thus in the popliteal region, pus may descend into the leg, separating, as by dissection, muscle from muscle; or it may ascend in the thigh, separating in the same way the muscles, as far as the groin; below it finds a firm, dense tissue; above the tissue is loose and lamellated; and this last is the direction it takes, under the abdominal integuments. It follows the cellular sheaths of the vessels and nerves, and so transports itself from one point to another. Should its source be some part of the vertebral column, it may be seen dissecting the nervous plexuses, and the various aponeurotic layers, and collecting definitely at some part of the thoracic or abdom-

inal parietes or at the groin; or sometimes it insinuates itself into a muscle, follows its vessels or nerve, and transforms it into a veritable sponge filled with pus. Again, it may be inaccessibly confined, as in a capsule under strong and dense aponeuroses, when, taking advantage of some minute interstice, it opens out among the fibres a way for itself, which it enlarges by virtue of its corrosive qualities, and goes to invade a new region which you would have supposed secure from any attack. It is thus you see suppuration, confined at first, it may be, to the axilla or thoracic parietes, penetrate the thoracic, peritoneal or articular cavities, &c.

The different tissues attacked, or exposed by a wound, have almost always different degrees of tendency to cicatrize. The skin cicatrizes more readily than muscle, bone, tendon, &c. Hence some wounds close externally whilst deep-seated suppuration is in full activity. Hence wounds closed externally and apparently cured may all at once give issue to an unexpected flood of pus. This is often seen after extirpation of the breast; and there is at this moment an unhappy example of this in the ward for females. This variety of tissue, and this suppuration, which is but too often a result of treatment of wounds by the first intention, baffles the hopes that had been entertained of converting a vast wound into a shut cavity, where the bleeding surfaces, placed directly in contact, deprived of communication with the external air, would have but to adhere to each other, or where at least the suppuration, as in the naturally shut cavities, would not be exposed to these changes that contact with atmospheric air produces.—*Medical Circular, April 28th.*

PROCEEDINGS OF SOCIETIES.

NEW YORK PATHOLOGICAL SOCIETY.

[Reported for the MONTHLY by E. LEE JONES, M.D., Secretary.]

Regular meeting, January 13, 1858. Dr. J. R. Wood, President.

Dr. Alonzo Clark exhibited a portion of a membranous material that was withdrawn from the bladder of a woman shortly after parturition. The specimen was brought to him by Dr. Weeks. It is of interest in connection with one that was exhibited two or three meetings since by one of the members. The question then arose as to the character of this membrane. He supposes this to be an analogous membrane. In regard to this specimen, on one surface it is rough and irregular, and on the other, smooth and of a whitish color. On the rough surface there is no adventitious matter, while on the smooth

surface there is an abundance of the triple phosphate, apparently incorporated to a considerable extent in the tissue. The microscopical character of the smooth surface is granular and amorphous, while that of the rougher surface is fibrous, the fibres exactly corresponding in character to the fibres of the healthy bladder. The rougher surface looks very much as if this tissue was the result of the splitting of the tissues of the bladder. He could but think that this was the mucous surface—that the mucous membrane had been separated, and a portion of the areolar tissue, that binds the mucous membrane to the muscular, had been separated with it. The history of the case is substantially this: On the first of December this woman was found to be suffering from some pains about the pubic region, being then pregnant. He had forgotten at what period, but at all events it was at such time that the discharge was regarded as an abortion. Very soon after this the waters came away, and for several days subsequently (8 or 9 he thought) she suffered from annoying and teasing pains. Finally the fœtus was discharged, and after this she continued to have considerable pain in urination, and was soon unable to pass her urine at all. Dr. Weeks was obliged to use the catheter; he would sometimes draw away something like a pint at a time. Shortly after he found it would fairly enter the bladder, would move in a certain space, and no urine would flow; and in two or three instances, in pushing the instrument on and moving it about, it passed through something. On this membrane will be found two holes; in another portion of the membrane, in the possession of Dr. W., there are two more holes. After a certain length of time, this difficulty of urination, pain, &c., continuing, she passed, for the most part, urine, a specimen of which is here presented, kept two or three weeks. The sedimentary matter is entirely pus and blood corpuscles that have lost their color. The deep color of the supernatant fluid is made by the coloring matter of the blood. This was the kind she passed continually. At length the nurse observed that something was wrong. The doctor came and found the urethra filled with an animal-looking tissue. He took hold of it with the forceps and gradually drew it down until he could get hold of it with his thumb and fingers, when he slowly withdrew it, giving the woman great pain from the distension of the urethra. From this time she was able to pass her water without a catheter, and for a considerable time its passage was not very frequent. The appearance of the urine changed after a little while from this deep muddy-looking fluid to something nearer its natural color. This urine was passed some two or three weeks after the membrane was withdrawn. It was

procured for him for the particular purpose that he should ascertain if there were any cells in the sedimentary matter that would indicate separation in the tissues of the bladder. He saw none. The woman has gone on very well—her pains have decreased, but she can contain only a little urine in her bladder, and urinates about every two hours. At first she got along very well, but now the bladder seems to be unwilling to contain any considerable quantity. What its state is, is not known. He believes that the examination of this specimen will pretty decidedly lead to the conviction that it is some of the natural tissues of the bladder, or some other viscus, (it could hardly be anything besides the bladder,) and not an adventitious or false membrane, in this respect confirming the impressions of the gentleman who presented a similar specimen some time since.

Dr. J. R. Wood observed that *Dr. Krakowitzer* presented a case very much like this a short time since, and a committee was appointed to investigate its character. The committee consisted of *Drs. Dalton, Gouley and Sands*.

Dr. Draper was present at the examination, and stated that the appearances were exactly those which *Dr. Clark* described. The smooth surface was covered with white deposit of the phosphates, considerable granular matter; the rough surface had a coarse areolar structure. It was, however, so much changed at the time of its examination, no positive opinion could be arrived at as to its nature. No epithelial cells could be discovered. It was *Dr. Gouley's* opinion that it was a false membrane, and not the mucous membrane of the bladder.

Dr. Peaslee.—Was there any trace of vascularity in either of these specimens?

Dr. Clark.—It will be recollected how difficult it is to find vascularity where there was no blood in the tissues. As these were separated from the bladder, and had been macerating in the urine for some time before they were discharged, they would hardly be likely to contain any blood; therefore it would be impossible to distinguish vessels from other organized tissues.

Dr. Peaslee knew it was difficult to make out vascularity in such cases, but still, if that were possible, it would demonstrate clearly that this was part of the tissue of the bladder. His impression was, that the membrane thus extruded could be none other than a part of the original tissue of the bladder. He did not know why sloughing of the membrane of the bladder could not as well take place as sloughing of the skin; the subsequent symptoms which have occurred are those

which should have been expected to have taken place. When the other specimen was introduced here, he suggested that probably it was a false membrane; that it did not come from the bladder, but from the vagina. He asserted this on general principles, because he could not see how false membranes could be found, under any ordinary circumstances, in the bladder, unless there was total suppression of urine. He could conceive how it can form here, when it is in a state of rest, as well as in any other tissue of the body.

Dr. Markoe exhibited a specimen of *hip-joint disease*.

Dr. Finnell exhibited the *cerebellum and dura mater* taken from a German, 54 years of age. He had been disputing with some neighbors about some trivial affair, and got in a great passion—during which excitement one of the neighbors struck him a slight blow on the head with the handle of a small axe. Very little was thought of the injury, and he retired to his room; soon after, however, he complained of lancinating pain in the head. He directed his son to obtain him a cooling application for his head, but before the son returned with the water he was dead.

Autopsy.—There is seen a rupture of the left lobe of the cerebellum, and a large quantity of coagulated blood in the substance of the hemisphere; this had passed out through the lacerated portion of the cerebellum, and lodged in the base of the brain. No other lesion was discovered. The question arose, whether this blood was the result of the injury, or of apoplexy. He would ask the members if any of them had seen apoplectic effusion in this situation, and if so, if this is such a case. In removing the *dura mater*, he found a large concretion branching down from the falx, about the size of a bean. It is a sharp spicula of bone. There is no surrounding inflammation.

Dr. Dalton doubted the existence of laceration, and stated that it could not be determined in the present condition of the specimen, but would like to ask *Dr. F.* whether the appearances, in its recent state, were those of laceration. I see (said he) what appears to be a laceration, which seems to be really nothing more than a convolution—an apoplectic clot, forcing itself in between; the convulsions would certainly be very apt to produce this condition. It seems very difficult to understand how the cerebellum could be thus lacerated by a blow.

Dr. Finnell thought there was no doubt of a laceration of the brain substance—that when he raised the cerebellum, he saw blood in the base of the brain.

Dr. F. also exhibited the *Cervical Vertebrae—Carotid Arteries in situ—together with Larynx*, taken from a man who committed suicide

by stabbing himself with a knife. He was a very intemperate man, and was laboring under delirium tremens. He went to bed as usual, at night, with his wife; he arose during the night, went to his bench, (he being a shoemaker,) seized a knife, plunged it into his neck, and returned to bed. In the morning his wife found him dead by her side. There was a large quantity of blood around the bed. The knife is seen sticking firmly in the body of the fifth cervical vertebra; the opening will be seen on the right of the larynx, passing just below the os hyoides, escaping the carotid arteries as they lie in place. He stated that this case was interesting in connection with the testimony given at the trial of Donnelly for the murder of Moses—whether such a wound, as found on the body of Moses, could have been suicidal. The question was discussed before the Society. Most of the gentlemen were satisfied that the wound could not be suicidal, on account of its severity, position, &c.; but here is a wound precisely like the one inflicted upon Moses. Had he seen this specimen before, he could have sworn positively whether it could have been suicidal or not. The wound was in precisely the same place, and death arose from the same cause, bleeding from deep thyroid artery.

Regular meeting, January 27, 1858. Dr. Peaslee, President.

Dr. Isaacs read the details of a case of *fracture of the skull*, presenting many points of interest. The patient was brought before the Society. The case occurred in the practice of Dr. George Cochran, of Brooklyn.

Dr. Enos showed a *vesical calculus* removed from a boy 11 years of age, who had suffered from symptoms of stone for the last eight years. When he entered the Brooklyn Hospital, two months since, his pulse was 164. The water was very copious in quantity, of very low sp. gr. It abounded in crystals of the triple phosphates. It was impossible to introduce anything into the urethra unless chloroform was administered, so great was the irritability of the parts. He readily detected the stone both by sound and examination per rectum; it was judged to be pretty large. On account of size of the stone the bilateral section was resorted to, and the instrument invented by Dr. A. Post for that purpose was used. There was considerable difficulty in extracting it. In about twenty-four hours after the operation he passed his water entirely by the urethra, which, however, only continued till the next day; it was then passed through the wound for nearly two weeks; then the bladder would retain the water for an hour or two—part would go through the perineum and part through the urethra. About the time that he began to retain his water there were symptoms of

inflammatory trouble about the bladder; the bowels became distended, somewhat tympanitic; there was tenderness on percussion, the pain referable to the umbilicus. The pain over the bladder was very acute indeed. The pulse, which had been down since the operation to 100, now rose to 140. The only marked difference between these symptoms and those of inflammation was the fact that steady pressure could be tolerated over the abdomen. These symptoms gradually subsided, until now he is nearly well. The central portion of the stone is almost entirely made up of uric acid, the outer portions are somewhat phosphatic. Weight 1 ounce and 3 grains. It measured in largest circumference $4\frac{1}{2}$ inches.

Dr. James Wood presented a specimen of *impacted fracture of neck of femur*, in which there was perfect union with the loss of the neck.

The subject from whom it was taken was 57 years of age at the time of the accident, May 20th, 1857, and although not very robust, still enjoyed very good health. He was not strictly temperate, neither could he be considered intemperate, judging from the history he gave me of his habits. When a young man he contracted a chancre, which was treated as soon as it was noticed, and not followed by any discoverable constitutional symptoms that I could ascertain.

He came under my charge May 23d, nearly three days after the accident, of which he gave the following account. While descending a stairway he slipped a few feet and fell, striking the postero-lateral portion of his left trochanter major against the projecting edge of one of the steps. On recovering himself, he found that he could not move his left lower extremity, but supposing that he had suffered no serious injury at the time, he did not seek surgical advice, as he thought that he would recover in a few days and be able to resume his trade, which was shoemaking. Finding, however, that he grew no better, he came under my charge, presenting the following symptoms, viz.: Shortening of the left lower extremity a little over one inch; eversion of the foot; inability to elevate or rotate the limb inwards; and very well marked osseous crepitus when extension and rotation were made. He was examined by a number of medical gentlemen, who were all of the opinion that a fracture of the neck of the bone existed, but differed from me in not expressing any opinion as to its seat. Taking into consideration the age of the patient, and the small segment of a circle described by the trochanter major, when extension and rotation were made, I supposed it to be a fracture of the neck within the capsule.

The only treatment adopted for the first three days was to place the limb in as easy a position as possible. On the 26th of May, a

long strip of adhesive plaster was applied along the inner and outer aspects of the leg and thigh, forming a loop a couple of inches long below the foot. The limb was then snugly bandaged and secured to a splint that extended from a body belt to a point eight or ten inches beyond the foot. Extension was then made by connecting the screw in the lower end of the splint to the loop of adhesive plaster, and counter extension made with a perineal pad that was attached to the upper end of the same splint. The extension was increased from day to day, keeping the limb nearly its natural length, until the end of the fourth day, when the perineal pad, although soft, had chafed him so much that it had to be removed. He was then placed in an apparatus invented by Dr. Burge, with which counter extension is made over the tuberosities of the ischia, and extension by means of the screw at the lower part of the long splint. In this apparatus his pelvis was held secure and at rest, being confined between the splint on one side and a movable support on the other.

On June 24th it was found necessary to re-dress the limb, as the bandage had loosened over the leg, and the plaster began to slip. At this time there was half an inch of shortening by measurement. He bore the treatment very well. No change was made from this time until July 19th, when the apparatus was dispensed with. The limb still continued half an inch shorter than the uninjured one, and, judging from the examination made at the time, union was supposed to have taken place. Having complained of considerable pain in the joint when moved or left unsupported by a pad behind the trochanter major, a blister was applied, with the supposition that some inflammation existed within the capsule. This was followed by relief for some days, after which time it was considered proper to repeat it, and so on at intervals for some weeks, at the end of which time he could forcibly push himself up in bed, by placing the foot of the injured limb against the foot rail of the bedstead.

He never recovered, however, so as to be able to walk about, although he would stand on the limb whenever he was desired to do so.

On the 19th of August he was attacked with acute bronchitis, which soon yielded to treatment. As some cough and expectoration continued, a thorough examination of his chest was made, and it was found that tubercles were softening in the anterior portion of both lungs.

With but few exceptions he was confined to his bed until the time of his death, January 23d, 1858.

Post-mortem 23 hours after death. Lungs found extensively diseased.

The accompanying specimen was then removed, and macerated in alcohol and water. As I desired to examine it a little, I divided the capsule close around the border of the acetabulum, and cut through the ligamentum teres. I made a horizontal section through the head, neck, and trochanter, thinking that a clearer view could be obtained than if it was made vertical.

Dr. Markoe presented a specimen of *fibrous cellular tumor* from the *anterior wall of vagina*. The tumor was taken from a patient 40 years old, a widow, of excellent general health, though bearing some scars in the neck of scrofulous disease of the glands. She had perceived it as a distinct tumor about nine years, but from the time of her marriage, at the age of 17, she thought there was something wrong about the orifice of her vagina, because penetration was impossible, and never was fully accomplished until she had been married seven years; and in the mean time the constant attempts at connection gave her a vast deal of pain. At this time conception took place, and she bore a living child, without any unusual difficulty, and for some time after its birth connection was more easy, but the impediment gradually returned, and has always since existed. The tumor from its commencement has been painless, and only during the last few years has it become inconvenient by its position, not only by its protruding constantly from between the labia, but by its pressure backwards, occluding the mouth of the vagina, and causing a retention of the menstrual discharges. The tumor before removal appeared to be about the size of a small hen's egg, and at first sight appeared to protrude from the orifice of the vagina. The urethral orifice, however, was seen on its upper surface, drawn out by the protrusion of the tumor, and the opening of the vagina was behind and before it. The finger passed into the vagina, recognizing the tumor as pressing down the anterior wall of the passage, and reaching up some distance along the urethro-vaginal septum; a catheter passed into the urethra could be felt by the finger in the vagina, just before its entrance into the bladder. It appeared clear, therefore, that the tumor occupied the space between the mucous membrane of the urethra above, and of the vagina below, and the history seemed to show that at its first appearance it projected backwards, so as to occlude the vagina, and that as it gradually grew larger it escaped from the vagina, and enlarged externally. Its surface was covered by healthy mucous membrane, not adherent to the tumor, and by great care and cleanliness it had been kept from excoriation, till a few days before the operation. The removal was effected without difficulty, by an oval incision beginning about one-third of an

inch below the orifice of the urethra, and coming to a point near the posterior part of the tumor. By carefully carrying the incision down to the substance of the tumor, it was enucleated easily with the finger and the handle of the scalpel. Before the operation it was difficult to decide whether the tumor, which was firm and elastic, fluctuated or not. This doubt ceased the moment it was released from its coverings. Its elasticity ceased, and it became flabby, resembling in all external respects a rather soft, fatty tumor. It was covered by a perfect, but delicate capsule. On section it presented a homogeneous surface, of a light pinkish color, faintly intersected with lighter colored bands and fibres. Under the microscope it presented the appearance of fibres banded together in parallel lines, and somewhat interlacing with each other. There were a good many nucleus-looking bodies scattered about, but I could not decide whether they belonged to the fibres, or were accidentally placed among them. The appearances to the naked eye, and the history and position of the tumor, corresponded very nearly with Paget's fibro-cellular tumor, and the microscopical appearances were sufficiently similar to induce me to regard it as belonging to that class.

EDITORIAL AND MISCELLANEOUS.

The four last meetings of the Academy of Medicine have been of a decidedly scientific character—a great improvement as compared with some former meetings, when the time has been frittered away in trivialities, which are neither edifying nor profitable. In all such societies there are some who are constantly seeking for notoriety, whose audacity is boundless, but whose only opportunity for making themselves conspicuous occurs in connection with some question as regards the mechanism of organization. They are strong on motions and amendments, and quasi ethical questions, especially if there is a chance for the indulgence of mean insinuation or the gratification of any petty malice; but they are only “dumb dogs” when scientific questions come up.

At the regular meeting, after the transaction of the ordinary business, Prof. Dalton read the interesting and elaborate paper on the structure of the placenta, which will be found in the first part of this number of the MONTHLY. At the conclusion of this paper, which was

received with much applause, the President called upon Dr. Barker to make some remarks.

Dr. Barker said he had been an interested listener to the valuable paper, which had given so clear a description of the present received doctrines as to the structure of the placenta. In the main, they were the same that he had been accustomed to teach for seven or eight years past.

He believed that it was now established beyond a doubt that the decidua is only the hypertrophied mucous membrane and its follicles, and not a new membranous exudation, and that the placenta is developed by the intermingling or interlacing of the vascular apparatus of the decidua with the villousities of the chorion; and that while there is no communication between the maternal and the fetal vessels, the placenta is really made up of two distinct parts, the uterine or maternal vessels and the umbilical or fetal vessels. He (*Dr. B.*) was especially interested in the new mode of demonstrating, by means of the blow-pipe, the direct communication of the uterine vessels with the placental sinuses. These experiments were entirely new to him, and he thought that they had an important practical bearing. It is well known that in certain cases of placenta previa, when the hæmorrhage is so severe as to jeopardize the life of the mother, *Prof. Simpson* has proposed to separate the entire placenta for the purpose of arresting the hæmorrhage. Clinical experience has shown that this entire detachment is followed by a complete cessation of the hæmorrhage. *Dr. Trask*, of White Plains, in his prize essay on placenta previa, a paper which has received and justly deserves the highest encomiums, has conclusively established this by his statistics. This proposition of *Prof. Simpson* has been violently opposed by many eminent obstetricians, and the question as to the source of the hæmorrhage in placenta previa has been much controverted. Some have believed that where the hæmorrhage occurs in consequence of partial separation of the placenta from the place of its insertion in the uterus, the blood flows from the orifices of the exposed uterine vessels. Others have believed that it passes from the maternal vessels into the placenta by that portion of its surface which remained attached, and escapes out of the placenta by that portion of its surface which has become detached. Now, these experiments of *Dr. Dalton* show that the blood flows from both of these sources, from the denuded surface of the partially detached placenta, and also from the exposed uterine veins. We have also an anatomical basis for estimating the value of the new plan of treatment for placenta previa, proposed by *Dr. Barnes*, of London. Without stopping

now to discuss the arguments by which Dr. Barnes sustains his views, he would only mention the clinical fact, that in one case only had he had the opportunity of testing his plan, and in this case he was not successful in arresting the hæmorrhage. Much remains yet to be learned in regard to the physiology and pathology of the placenta, and he hoped the author of the interesting paper that we had heard this evening would continue his investigations. He would like to inquire of Dr. Dalton whether he had found fatty degeneration of the placenta a frequent occurrence. A few years since much was said of this pathological change as a cause of premature labor.

Dr. Dalton replied that he had not directed his attention to this subject.

Dr. Taylor made some remarks, the purport of which we could not gather.

Dr. Geo. A. Elliot, in reply to *Dr. Barker's* question, remarked that when *Dr. Barnes'* paper appeared he was Resident Physician in the Lying-in Asylum, and engaged with *Dr. Van Arsdale* in making some researches on the urine of pregnancy, and that with the assistance of that distinguished microscopist he had examined a number of placentæ during a period of some four months, without finding the evidences of the fatty degeneration alluded to by *Dr. Barnes*.

Dr. Alonzo Clark said that he had made microscopic examinations of the placenta, and had found distinct evidences of fatty degeneration.

The Journal of Dr. Simple, Practitioner of Medicine, Paris.

(CONTINUED.)

January 16. Madam Laguerre, a very amiable and well-known nurse, called to present her compliments and to ask me to recommend her to my patients. She has a good figure, and I am not surprised that *M. Roux*, whose death she still mourns, patronized her largely. I shall employ her notwithstanding *Benoit's* proverb, "Whoever has a nurse has a viper."

January 17. I occupy the second story of the house where I live. What was my surprise this morning on going down stairs to see the word DOCTOR shining on a copper plate on the door leading to the first story. I was even informed by the *concierge* that the first story was hired by a confrère. This is not pleasant. *Benoit* coming in says, that not having any clause in my lease making a stipulation in this respect, the landlord has the right to do so. Still there is sure to be confusion, and this will be to my disadvantage. *Benoit* consoles me by saying that my position is more elevated than that of my confrère.

January 18. The disagreeable effects of this arrangement begin to be felt. Three or four persons have rung at the lower door, thinking they rung for me. Have I not the right to put a sign on the most prominent part of the entrance, indicating that Dr. Simple lives in the second story?

January 20. Madam Laguerre, whom I have put in charge of a young typhoid patient, pleases me. My neighbor, below me, is a homœopath. Benoit would persuade me that the danger is no greater. He says there is only one way of exorcising him, and that is by bringing the *concierge* into my interest. I reject this method indignantly.

January 21. Benoit, who has no scruples, and wishes to mix himself up in my affairs in spite of me, went and offered 50 francs to the *concierge* to attend to my interest. He replied that the physician in the first story was more generous, and had offered him 100 francs. There, that's well done! Am I sufficiently humiliated?

January 23. Will Benoit's proverb prove true? Madam Laguerre has constantly made mistakes and blunders. I ordered twenty leeches to the mastoid processes, and she put on only a dozen, applying them to the anus, with the excuse that in young girls it is necessary to avoid making scars behind the ears. I prescribed milk mixed with barley water; she gave only the barley water, pretending that the milk was too nourishing, but the milk disappeared. I advised the use of Peruvian bark, Malaga wine and cocoa, four teaspoonfuls a day. The half-pint bottle was dry in twenty-four hours. They gave her two hearty meals a day, with two bottles of wine, coffee in the morning, and a substantial lunch at night, and she complains that she is dying of hunger, and that I had sent her to a hovel. Has she not scolded about the seidlitz water, pretending that castor oil is borne better? Has she not dared to say that the great physicians do not treat typhoid fever in this way? Fortunately I am well anchored in this family.

January 24. Madam Laguerre has excited distrust; they ask for a consultation for the girl with typhoid fever, though her disease is mild. They propose Dr. X. This is Madam Laguerre's choice. What more can Dr. X. do for a simple disease which approaches a happy termination?

January 25. Dr. X. behaved very well. He greatly approved of my diagnosis, prognosis and treatment. However, he has substituted chicken broth for the porridge, magnesian lemonade for the seidlitz water, injections with oil instead of flax seed, and appears to attach great importance to these unimportant changes. Madam Laguerre appears triumphant. This woman decidedly displeases me.

January 26. Six months since I had the misfortune to lose a patient who resided in the *Rue d'Astorg*. The widow, who inherited a handsome fortune from her husband, not having yet paid me, I thought that after six months I might be permitted to remind her of my care of her husband, and I wrote her a very polite note. In the course of the day I received the following billet:

"Madam Widow X., to whom you so cruelly and so soon recall a

painful loss, finds it impossible to attend to your account, of which you have besides given her no items. Be good enough to make a note of it, and send it to her on her return from the journey she is just about to make to Italy."

Billet without signature, initial paper, open work, perfumed, and with a large black border.

Benoit, who knows everything and sees everything, says that this widow, whose sorrow prevents her from paying her husband's physician, has repaired her charming little house in the *Rue d'Astorg*, from top to bottom. It is, probably, for the purpose of removing every recollection of her loss.

Jan. 27. My little typhus patient gets on better and better. Certainly, said her mother to me this morning, since Dr. X.'s treatment was adopted the disease has changed its character. But, madam, I replied, there was no more danger before the consultation than there is now. Oh, I saw the sign Dr. X. made when he felt of the bowels. What sign did he make? The abdomen has remained soft, the gurgling was scarcely perceptible, there was almost no diarrhœa, but few spots on the skin, and little disturbance of the mind. This, said madam Laguerre, did not prevent Dr. X. saying that it was necessary to do something more energetic. But the means which he advised were identical with those which I had myself used.

It was of no use. I saw that the family was prejudiced, and that all the honor of the cure would be given to Dr. X.

Benoit on this occasion expounded to me a long theory on consultation. "It is necessary," said he, "to listen with great attention to all that is said, and to let nothing pass which can raise a doubt in the minds of the family." It is necessary—it is necessary for so many things that my poor head can never remember them.

January 28th. There are some compensations for professional trials. Some months since, I attended for a grave acute disease a young woman, to tell the truth, a beautiful sinner of the *Rue St. Lazare*. Although I did not expect anything from this patient, living so improvidently; passing before her door one morning, I went up to see her. She was in a bath perfumed enough to make one dizzy.

"Ah, is that you, good Doctor?" she cried; "I have not forgotten you, be sure of that. I owe you much money, do I not? I will pay you, don't be uneasy. What do you think? this frightful disease from which you saved me, had so changed me, I was so thin, so pale—you understand, Doctor."

"But, my child, I did not come to dun you; passing before your door, I wished to see if your health continued, that was all."

"I get on charmingly, my good Dr. Simple. You are very kind to make me this little visit. There, let us make everything pleasant—breakfast with me."

"Thank you, thank you, my child, I never breakfast; and I leave you, delighted to find you are so well."

"Yes! have I not changed very much?"

"You are more charming than ever. Adieu! adieu!"

"I shall not forget you, Dr. Simple, and perhaps you will have news of me sooner than you think."

I was in my private room, when about three o'clock Frances admitted a handsome, elegant, and, upon my word, a very charming young man.

"Have I the honor to address Dr. Simple?"

"Yes, sir!"

"You attended Mademoiselle Camille, Rue St. Lazare?"

"Yes, sir!"

"I am permitted by her to have the honor of thanking you, sir, for the excellent care which you have given her."

The handsome young man rose, and placed a small roll upon my mantel-piece, and departed.

This little roll contained ten golden Napoleons. I should not have had such windfalls at Tartas.

Half an hour later, Frances introduced a gentleman of a more mature age, decorated with several many-colored ribbons, perfectly dressed, who said, with a slightly foreign accent, "is this Dr. Simple to whom I have the honor to speak?"

"Yes, sir!"

"You attended Mademoiselle Camille, Rue St. Lazare?"

"Yes, sir!"

"Mademoiselle Camille charged me to offer you her thanks—"

"But, sir—"

"She has one regret—which is, that she cannot show herself more grateful;" and the gentleman, rising, laid a roll upon the mantel-piece. I was astonished; if I speak, said I to myself, I shall disclose—but yet, I cannot keep—

While I made these reflections, the decorated gentleman had already gone. The roll contained fifteen Napoleons. There was no time for hesitation—I took my hat and went to Camille's house with the two rolls. She saw me at a distance, and cried out,

"Has he done it well, dear Doctor?"

"But *they* have done it too well—"

"What! did they both go there?"

"Both!"

"Ah! that is charming, that is charming;" and this giddy girl burst into laughter.

"I will tell you all about it," said she; "they are two real gentlemen, are they not, Doctor? I told both of them that I could not admit them to my society till they had paid for me a debt of affection. I told them of you, and of your kind care, and see—"

"All that does not concern me; here are two rolls which I have received, take whichever you will—"

"Fie, Doctor, for what do you take me? It is not too much for all you have done for me. Besides, there is a way of reconciling the whole; keep the first roll for the past, and the second for the future. I shall not always have so good an opportunity."

It is all the same; one of the rolls weighs upon me, and I will send it to the Association of Physicians of the Seine.

January 29th. My neighbor, the pharmacist, who wishes to be friends again with me, has sent me six bottles of Malaga wine, with which he says he prepares his laudanum. After all, perhaps he is not a bad man.

January 30th. I received an invitation to dine with the Surgeon X., who treated my patient in the Rue Duphot. Benoit had an attack of ill humor on reading the invitation. "What," said he, "this surgeon has taken away your patient, and he thinks to pay you by inviting you to dinner; refuse properly, without compliments."

I wrote at his dictation, "Doctor Simple cannot accept the invitation of Doctor X."

I think this note very dry.

"One of two things," replied Benoit, "this confrere fears you or esteems you. If he fears you, he will perceive that you appreciate his indelicate act, he will fear you more, and will not repeat it on your account; if he esteems you, he will esteem you more for refusing his dinner."

On this subject of dinners Benoit has made a most vehement tirade. He must write it out for me, that I may enter it in my journal.

January 31. I was called to an old lady in this quarter of the city. She is not exactly sick, but she desired to make provision for a physician in case of accident, and wished to know him beforehand.

"How much do you charge a visit," she said to me.

Considering the appearance of ease in the apartment, I thought I was very modest in saying three francs.

"Three francs!" replied the old lady; "but Professor Z. never charged me but two francs. He now lives so far off that I wish to have another physician, but at the same price."

And it is so. How can Prof. Z., so very well known, so sought for, come here, and at so low a price poach upon the lands of the poor practitioners of the quarter? Benoit thinks it is very mean.

Benoit has produced a great excitement in my mind. He wishes me to marry. I have listened to Frances' remonstrances all day, who thinks that I shall never be as happy as I am now. Perhaps she is right. I do not know what I have done all day. Benoit wishes to present me to-morrow.

February 1. In the possibility of my marrying I set about collecting my dues. I have written to-day to one of my patrons, whose wife and three children I have taken care of for three years, and of whom I have never asked any fee. So polite are they to me, I am invited to dine at least once a month, and there is no family fete at which I am not a guest.

February 2. I have to-day received the following answer to my demand:

"Doctor—It is with great surprise that I have received your bill, amounting to 140 francs, for twenty-eight visits made to my wife and children during the last three years. You have forgotten, and you

compel me to remind you, that during those three years you have taken dinners at my house which amount to a larger sum than that which you demand of me. These are the items:

30	Dinners, at 3 francs each.....	90 francs.
3	Extra dinners on my wife's birth day, at 6 frs..	18 "
3	" " " own " " ..	18 "
1	" " my daughter's first communion	6 "
3	" " the distribution of prizes to my children, at 6 frs.....	18 "

Total.....150 francs.

You owe me, therefore, 10 francs, which I beg you to send to me, for my family will not continue to employ a physician who is so forgetful. I have the honor to be, &c.

X., Button Maker."

Benoit almost split with laughter at this letter, which did not seem to me to be very laughable.

"Did you go to dine there without being invited?"

"Never; and on the contrary every time I went to dine I had to give advice to all the family, and even to the domestics."

"And have you reckoned these consultations in your bill?"

"Not at all, I have counted them visits only which I made on days when I did not dine there."

"Then he must be sued, and I will attend to it."

February 3. I related this experience to my future father-in-law, a linen merchant in the *Rue St. Fiacre*. Benoit says I did wrong, and that the father-in-law asked him in a very cold way if such mishaps often came to me. He added, that a physician's income did not appear to him to be very certain. He asked questions upon questions, which embarrassed Benoit even.

February 4. I received to-day a very singular visit and a still more singular proposition.

A man rather gentlemanly in his appearance, though his language was somewhat common, attracted, he said, by my reputation as a practitioner and as an honest man, made this statement to me:

A learned naturalist, sent on a mission to South America, had discovered in his travels a marvelous remedy for the cure of the gravest and most desperate diseases of the lungs. He quoted cures by hundreds, and in Paris even some wonderful cures had just been performed. He kept the remedy a secret; but as he was not a physician, could not use it; that is to say—profitably. He desired to find a physician who would allow him to make him rich. A company of capitalists had been formed; a superb apartment was already rented in one of the finest quarters of Paris. Rich furniture decorated it. They were willing to spend from 30 to 50,000 francs a year in advertising, if it was necessary. They would offer to an honest physician who was sufficiently known, this elegant lodging, domestics in proportion, a carriage, and a quarter part of the receipts, which they supposed would exceed 150,000 francs a year.

It was this position and this profit which the gentleman came to offer to me.

The blood rushed to my forehead during the discourse of my impudent visitor.

"And you think," said I to him, "that an honest man can accept such a proposition?"

"Why not? There are in this place, Paris, affairs of this kind which are very profitable. Do you wish me to mention some of them?"

"The *philanthropico-humanitarian pharmacy*, where the advice of the physician is given gratis, but where the remedy is sold at a very high price—a partnership.

"That *female physician* who takes up so much room on the fourth page of the papers—a partnership.

"That *office for consultation* in the name of a physician who has been dead these twelve years, the placards of which are found posted even on the pyramids of Egypt—a partnership.

"That —"

"Hold, sir! will you be so kind as to go and say to those who sent you that they have done me the greatest injury by daring to think me capable of accepting your proposition?"

This time I received the unreserved congratulations of Benoit, who advised me, however, not to say anything of the affair to the future father-in-law.

"Linen drapers do not understand these professional delicacies. He would only see one thing, namely, that you had refused 25 or 30,000 francs."

February 5. I begin to be known to and appreciated by the authorities of my quarter. My commissary of police wrote to me this evening, to invite me to go to-morrow morning to make the post-mortem examination of a person accidentally killed in a neighboring street. I am flattered by this preference.

February 6. What a horrible sight—what terrible labor! It was a poor devil, who had been dead at least three weeks, and the body was in a condition of complete putrefaction. I passed three hours in this poisoned atmosphere. It required more than an hour to draw up my report. I shall be allowed six francs for it.

February 7. The commissary of police thanked me for my report. He wished me to go to see a person in Amsterdam Street, who had been knocked down by a carriage and had entered a complaint. The individual had only a slight scratch on the leg, and I made my report accordingly.

February 8. The wife of that man came to my house to-day to abuse me. She told me that no doubt I was a stockholder in the carriage company, and did not wish they should obtain damages from the corporation.

February 9. One of my patients in Rumford Street had an attack of apoplexy last night. They ran for the nearest physician, and then sent for me. I was urged to go and see the patient. I had scarcely

entered the room when my *confrere* of the night presented himself. I think he should have waited till I sent for him. He wished to consult with me. He asked at what hours I would see the patient; in short, he intends to join me in the treatment.

This *confrere* embarrasses me. The family is not very well off; one physician is enough—why should he impose two upon them?

Fortunately, the husband of the woman came to see me, and asked if the situation of his wife was such as to need the assistance of counsel. I told him it was not. Treat my *confrere* properly, said I; thank him for his skillful care, and send him 20 francs for his night visit and 10 francs for the call this morning. It appears to me that, considering the circumstances of my patient, I have acted according to professional etiquette.

February 10. My *confrere* of the night is not satisfied. He wrote me an angry and offensive letter. I shall not answer it. Benoit, whom I have consulted, says that I have done as I ought. How suddenly an enemy has been added to my list!

February 12th. One of my friends from Tartas came to pass the festival days in Paris. Upon arriving he was attacked by the influenza, and sent to find me. That was not very easy, as he told me. In the hotel where he was, no one was willing to go on the errand. He is not the physician of the hotel, they replied, and accompanied their answer with great praises of Dr. X., with whom all travellers were very well satisfied.

Benoit tells me that, in fact, every large hotel not only has its physicians, its surgeons, and its druggists, but also a sort of medical brokers, on the watch for sick travellers coming to Paris for advice.

On this occasion he told me of a distinguished Russian lady, who came to Paris for advice concerning a tumor of the breast, and took rooms at the ——— Hotel. On the first day this lady found on her table the visiting card of a surgeon. The next day there was the account, published in a medical journal, of a brilliant and successful operation performed by this surgeon. On the next day an invisible hand had placed in the lady's apartment a book published by this surgeon. A day later a more direct step was made. A woman came to her under pretence of selling her some laces, and in the conversation alluded to a tumor of the breast which she had had removed lately by the same surgeon. This management succeeded so well, that this Russian lady, who came to Paris to consult Messrs. Jobert and Velpeau, was well and duly operated upon by Surgeon X., of whom she had never heard a word said.

Total, 6,000 francs, (1,000 of which go to the invisible hand,) and a foreign decoration.

— Medical science has recently met with many losses in the death of several of her brightest ornaments. We chronicle those whose names are familiarly known to both continents.

Dr. Robert Hare, formerly Professor of Chemistry in the University of Pennsylvania, died in Philadelphia, May 15. He was born,

it is said, in 1781, and was consequently seventy-seven years old when he died. Dr. Hare was well known throughout the scientific world as a natural philosopher of great eminence. His inventive powers were of the highest order; the reputation which he gained at an early age, and which was increased by after inventions, was owing in a great measure to the exercise of his inventive genius in the appliances of the Chemical Laboratory. In 1801, he entered the Chemical class in the University of Pennsylvania, and soon after he developed the oxyhydrogen blow-pipe, the invention of which was disputed by another experimenter in Great Britain, but is now unanimously conceded to him. Among other instruments which were invented by him during his long career as an experimental chemist, may be mentioned his modification of the common blow-pipe, and several important additions to the mechanics of galvanism. Among the latter are the calorimeter and deflagrator. In 1818, Dr. Hare was elected to the Chair of Chemistry in the University of Pennsylvania, to fill the vacancy occasioned by the transfer of the late Dr. John Rodman Cox to the Chair of *Materia Medica*. This position Dr. Hare occupied till 1847, when he resigned, and was elected *Emeritus Professor of Chemistry*, in the same Institution.

Dr. Hare was a brilliant experimenter, but not an agreeable lecturer upon his favorite science. Although he was the author of many articles which appeared in our scientific journals, he has left no work of magnitude behind him.

During the few last years of his life his mind wandered from the pursuits of legitimate science, and was much occupied by the hallucinations of Spiritualism.

— M. Chomel died at Paris, on Friday, the 9th of April last. His disease was protracted and painful, and for the last year has impelled him to withdraw from active professional life. An immense crowd of the physicians of Paris thronged the Church of St. Thomas Aquinas, where the obsequies were performed. Professors of the Faculty, many of them in their professorial robes, members of the Academy of Medicine in official costume, physicians, students, friends from all classes of society, were present to pay to him the last respectful homage. Several orations were delivered at his tomb in the name of the Faculty, the Academy of Medicine, and of the physicians of the Hospital.

M. Chomel was born at Paris, in 1788, being of course seventy years old at the time of his death. He was of a family distinguished in medicine for two generations, and proved himself to be worthy of

his name. At the age of 23 he was *interne* of the hospital, and laureat of the Faculty. At 25, he received the degree of Doctor of Medicine, and his thesis, entitled an "Essay on Rheumatism," which still remains an authority, gives indications of mental abilities. While still young he was attached to the medical service of the Hospital, and commenced a course of lectures on internal pathology, and enriched medical literature by several papers, by a work on Fevers, and by his Treatise on General Pathology. The last has now reached its *fourth* edition in French, and has been translated into all languages of Europe. In 1823, at the age of 35, Chomel was elected one of the twenty-four associates, whose appointment had then been authorized. Three years later he was elected by the Faculty to the Chair of Clinical Medicine, which was made famous by Laënnec, and had become vacant by his death. At the Hotel Dieu he divided public favor with Dupuytren, who had previously reigned alone. He thus maintained his position at the summit of renown till his death; and now throughout the world his loss will be felt and will be mourned.

— Prof. William Gregory, of the University of Edinburg, died April 24, in the fifty-fourth year of his age. He was descended from a family distinguished in the annals of science of his native city for more than two hundred years. His father was the celebrated Dr. James Gregory, Professor of Practice of Medicine in the same University. Dr. William Gregory occupied the Chair of Chemistry, and was more remarkable for his writings than for any brilliant discoveries.

— Professor Müller, of Berlin, the distinguished physiologist, died lately, in the fifty-sixth year of his age.

— Professor Robert Harrison, of the University of Dublin, died on the 23d of April, of apoplexy. His work on Anatomy, called the "Dublin Dissector," is universally known where the English language is spoken, and has ever been a favorite work with anatomical students.

— Dr. E. Geddings, Professor of Surgery in the Medical College of the State of South Carolina, has resigned his chair, after having occupied it, in connection with this College, for twenty-one years. Dr. J. J. Chisolm has been elected to this Chair.

— *The Maine Medical and Surgical Reporter* is the title of a new medical journal which is sent us from the State of Maine. The editors are Drs. W. K. Richardson and R. W. Cummings. It is published in Portland, and is to be issued monthly, each number to contain 48 pages. The *Reporter* is the only medical journal published in Maine.

—The vacancy occasioned in the Faculty of the Jefferson Medical College in Philadelphia, by the death of the distinguished Professor of Theory and Practice, the late Dr. J. K. Mitchell, has been supplied by the appointment of Dr. Samuel Dickson, of the Charleston Medical College. The resignation of Prof. Dickson of the Chair of Institute of Medicine in the latter institution, leaves a vacancy which has been filled by the appointment of Dr. P. C. Gaillard.

—We have received the May number, being the first, of a new medical journal published in Savannah, Ga., called *The Savannah Journal of Medicine*. Edited by J. S. Sullivan, M.D., and Juriah Harris, M.D.; R. D. Arnold, M.D., Associate Editor. It is a bi-monthly of seventy-two pages, of a neat typographical appearance, and bidding fair, judging from the character of this envoi, to take a prominent stand among the medical journals of the country.

—Savannah, Ga., also sends out another medical journal, entitled *The Oglethorpe Medical and Surgical Journal*. We have not seen a copy of it, but learn from our exchanges that it is a bi-monthly of 64 pages, and edited by Drs. H. L. Byrd and Holmes Steele, of the Faculty of the Oglethorpe Medical College.

—The Jenner Statue has been appropriately inaugurated, and now stands in Trafalgar Square, a tardy honor to the discoverer of vaccination. The fund for the erection of this statue was contributed by all countries, but the largest portion came from America.

—A new medical journal has just appeared at Athens, Greece, entitled, in Greek, "Journal of Medicine." It is to appear once a week, and is edited by Drs. Anagnostakis and Aphendoulis. This makes three medical publications which are now printed in Athens, the two others being monthly in their issue, and are called "The Medical Bee," and "The Esculapian."

—The British Government has bestowed upon Dr. Wynne, of this city, the great gold medal of that country, as an acknowledgment of his services in the cause of science.

Dr. Wynne, who is known to our readers as the author of the articles on the "National Hotel Disease," and the "Influence of the Gulf Stream upon the Summer Climate of the Atlantic Coast of the United States," which appeared in the vii and viii vols. of the MONTHLY, has long been identified with matters pertaining to public health in this country, and in this capacity has occupied many prominent positions. It is his attainments in this branch of science which won for him this distinguished honor.

—Among the many inventions of the age which are adapted to the comfort and elegance of every-day life, and yet have their immediate relations to our profession, we can signalize the Elliptic Spring-Bed Bottom. Beyond the mere result of affording a good bed for well people, that form of it called the invalid's bed is deserving the attention of physicians, and those who are furnishing hospitals. This bed bottom has the merit of durability and cheapness, of elegance and usefulness. It adapts itself to the movements of the body in any position, and can be so arranged as to permit the patient to change his position from a horizontal to a sitting one by a slight manipulation of an attendant. It can be applied to any species of bedstead, is so simple in structure that it cannot get out of order, and yet so elastic that it can well supply the place of a water bed in such diseases as require this form of bedding. For the healthy it is a luxury, and for the invalid a comfort.

—The Sydenham Society, so well known for many years for its publications, having been dissolved, a new society has been formed with the same objects and principles, under the name of the New Sydenham Society. It already numbers more than a thousand members. The annual subscription is one guinea, entitling the members to receive all works issued by the Society during the year. Dr. C. F. Heywood has been appointed the local Secretary for this state and city.

Books and Pamphlets received.

Census of the State of New York, for 1855. By Franklin B. Hough. From Secretary of State.

Contributions to Operative Surgery and Surgical Pathology. By J. M. Carnochan.

Transactions of the New York State Medical Society, for 1858.

Pathology and Treatment of the Paralysis of Motion. By J. P. Batchelder, M.D. From the Author.

On Intra-Capsular Fracture of Cervix Femoris with Bony Union on an interesting case of Urinary Calculi, illustrated by engravings. By Alden March, M.D. From the Author.

Researches on Primary Pathology and the Origin and Laws of Epidemics. In two volumes. By M. L. Knapp, M.D. From the Author.

Of Nature and Art in the Cure of Disease. By Sir John Forbes, M.D., &c. New York, S. S. & W. Wood, 1858.

Mind and Matter, or Physiological Inquiries. By Sir Benjamin Brodie, Bart., &c. New York, S. S. & W. Wood, 1858.

Ventilation in American Dwellings; with a series of Diagrams. By David Boswell Reid, M.D., F.R.S., &c., to which is added an introductory outline of the Progress of Improvement in Ventilation. By Elisha Harris, M.D., &c. New York, Wiley & Halsted, 1858.